The Psychological Record

a quarterly journal in theoretical and experimental psychological

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THE PSYCHOLOGICAL RECORD is a non-profit publication. It is published quarterly in January, April, July, and October, at Denison University, Granville, Ohio. Subscription price is \$4.00 a year (APA members—\$3.00; students—\$1.50).

With the permission of the Principia Press, Inc., THE PSYCHO-LOGICAL RECORD is a continuation of the journal formerly published under this title. Publication of THE PSYCHOLOGICAL RECORD

was resumed in January, 1956.

As presently organized THE PSYCHOLOGICAL RECORD publishes both theoretical and experimental articles, commentary on current developments in psychology, and descriptions of research planned or in progress. The journal is designed to serve a *critical function in psychology*. It therefore favors the publication of papers that develop new approaches to the study of behavior and new methodologies, and which undertake critiques of existing approaches and methods.

Articles should be prepared according to the form suggested for APA publications (APA Publication Manual) and submitted in duplicate to the Editor. The author cost per page is \$3.00. There is an additional author charge for cuts and special composition. Reprints are

available at cost.



SCIENTIFIC CREED-1961: PHILOSOPHICAL CREDO

WILLIAM STEPHENSON

University of Missouri

Introduction

A confession of faith is usually brief and firm. The present one is protracted and quixotical. This is because it is controversial: the concern is with theory and methods in the study of human behavior, in their logic-of-science, that is their metascience and methodological respects, and there are serious matters at issue. There should be no doubts about the kind of studies we have in mind because they fill the psychological journals, but it will be helpful to have a concrete example before us as the discussion proceeds.

With this in mind, Festinger's theory of cognitive dissonance (1957) has been chosen, and more specifically a paper by Lane (1959). Any of a hundred papers, and a dozen theories, might as readily have been taken for the purpose in hand: Festinger's theory, however, has an advantage that it involves self-psychological possibilities, a matter of central interest in methodological respects, so that it was chosen with this in mind as well.

The theory, it will be remembered, has three postulates, (a) that there is consistency between attitudes and behavior, (b) that conditions of 'discomfort' are like needs, and (c) that 'cognition' is antecedant to reduction of discomfort. Lane argued from these that because of the lung cancer scare cigarette smokers would be dissonant, and therefore would be more likely than non-smokers to stay away from a TV program dealing with lung cancer and smoking. He found instead that more smokers than non-smokers looked at the program, which he explained as "self-justification" on the part of smokers—thus calling upon Festinger's principle (c) a posteriori to account for the facts.

It is not necessary to worry about the initial erroneous deduction—indeed it is refreshing to find someone's hypothesis failing at the test. Instead, attention is drawn first to the fact that Lane was not making deductions about a particular person, X, to deduce whether or not he would shun the TV program. The concern is with 'general implications': and therefore if several hundred men are tested a statistically significant proportion will shun the program, according to the theory (or not, as the case may be).

F. V. Smith (1960), in his Explanation of Human Behavior wishes to have this statistical mode of testing recognized as a "definite type of explanation." It is neither truly deductive, he says, nor causal, but has "approximate resemblances" to these forms of explanation: but it fits the logic that an explanation is a conclusion to a tested hypothesis. Smith was impressed by the fact that much of the testing achieved so far in general psychology followed this statistical form; and equally so by Meehl's (1954) conclusion that statistical prediction is better than clinical (which is deductive and causal in form). The "definite type of explanation," however, is suspect in our creed. After all, one might have asked Mr. X whether he would shun the show, and there is much more than meets the eye in this riposte.

But, to return to Festinger's theory: in the second place (the first being the statistical basis just noted) there are other reasons for disquiet about such theories and hypotheses, in logic-of-science respects. Even if Lane's original hypothesis had been confirmed, it could not have been acceptable as evidence for anything, because there are other possibilities in the matter. The assumption that smokers are dissonant and non-smokers not is suspect at the outset. Smokers of regular cigarettes, like tobacco chewers, may really like what they do; smokers of filter-tip cigarettes alone may be the dissonant ones, wishing they could give up smoking, but unable to do so. Non-smokers who have recently given up smoking may be in pangs of discomfort, and the habitual non-smoker may be no less so to justify his abstemiousness. Nor it is difficult to grasp that much else in the domain of the self-Festinger's postulate (c)-might be significant besides self-justification. If a scientist hasn't considered all such possibilities, and falsified them (Popper, 1950), there can be little reason for anyone to take the original hypothesis seriously, even if facts confirmed it.

The same applies, *mutatis mutandis*, to the various theories and papers of which the above is merely typical.

It will be said, however, that a beginning has to be made somewhere, and that Festinger is surely on the right lines, and Lane with him, in putting their cards on the table. Nevertheless each of the postulates (a), (b) and (c) of the theory of cognitive dissonance is essentially a locus for many possibilities; instead of three, there are innumerable postulates. One could scarcely be justified, therefore, in presuming upon deduction to provide hypotheses. Note that what is at issue is not the "definite type of explanation" to which Smith draws attention, but a chaos of statistical proportions at the postulate beginning of things, before any deductions are drawn, or any hypotheses tested by statistical procedure.

One sees how difficult it was, therefore, for Lane to *predict*: It requires not only a statistical procedure to test hypotheses, but something of the same order to manipulate the postulatory-possibilities. In

the circumstances it would indeed be remarkable for the twain to meet in any proof of anything.

This, surely, is a severe problem indeed: how, in the face of chance at the beginning and chance at the end, can science be possible?

We have an answer, but no one seems willing to look at it. It consists of a radical methodological change. But this can be left for the moment, to look instead into Smith's scholarly critique of the psychological systems of the past fifty years, those of McDougall, Allport, Lewin, Freud, and Gestalt and Behaviorism (Watson, Hull, and Tolman). Smith concludes that an era of system-building is over, and that psychology is now engaged in testing hypotheses and gathering empirical facts. We agree as to substance, but not in the conclusions. The truth is that the systems are plainly failures, and it is derogation to end up, as it all began, testing unrelated hypotheses and gathering empirical facts. One should ask, however, why the systems failed, and it is not enough to blame it on the complexity of human conduct, or to seek a way out via statistical procedures of a dubious kind, played with chaotic postulates. Perhaps psychology, in all its fifty years of systems, has been putting the wrong questions to man.

This, then, is the preliminary setting forth of our area of concern, with a foretaste of its content. The creed follows. Clearly, if all has been a failure in a basic sense—it is not denied that important facts have been gathered in the course of the years, for there are plenty of fish in the sea—there are likely to be reasons for it. Some of these, it is hoped, will become obvious in the sequel.

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Science has no true philosophical implications, but depends on philosophy for some of its basic assumptions. Sides have to be taken, for example, as to whether our concern is with "this-world" philosophy, or "other-world," and it is particularly important for someone involved in self-theory to make clear that his concern is with this world. This is not to say that he is a materialist so much as an analyst of the world as it is given, and what is given are pots and pans, Toms and Harrys, ideas and feelings of people, and the like. A stand is taken with such a pragmatism, and therefore 'things' and not mind will be at the core of all else. Consequently, talk of consciousness and unconsciousness qua psychisms is not even theoretical for us (Stephenson, 1953).

But science has to be freed, also, of many traditional rationalisms, absolutisms and the like (Kantor, 1954). The idea that you can't "generalize" in behavioral science except in terms of large numbers of cases is of this arbitrary kind. We learn better by way of a logic of science, in its metascience and methodology respects. The former concerns inference, the meaning of probability, induction, deduction, and the like:

methodology has to do with principles of measurement, of theory construction, concept formation, models and instrumentation.

From the two branches, of metascience and methodology, comes the idea of science as hypothetico-deductive (Hull, 1943), or as purely interbehavioral (Kantor, 1954), or, as we believe, abductive (Buchler, 1950). Since our stand is with 'things,' including people with ideas, with respect to metascience and methodology there can be nothing to choose as between what a physicist and what we, as psychologists, have to say about procedures. However, analogies can be deceiving. The typical way of thinking of science at its best is as follows: the scientist observes material systems; measurements are made, correlations discovered, and empirical laws arrived at. That gases expand when heated, that liquids boil, that mice have cycles of eating activity-all such are empirical laws. Science at its best, we learn, is when empirical laws can be expressed mathematically, but these in fact mean very little until, besides the empirical laws, one has a unifying theory. High-level science is reached when, by way of a formal theory, which one represents by equations, one can draw deductions and arrive at expressions which agree with known empirical laws and point at the same time to others that were hitherto not suspected. The theory of universal gravitation is of this kind, obviously. This is hypothetico-deductive science, and there is wide agreement that it is science at its highest and best (Wisdom, 1952). Psychologists, therefore, have been busy stating postulates, deducing consequences, and subjecting these to empirical tests. The important matter, now, is not the discovery of empirical laws, but the more general assertion of a logical system, like geometry, consisting of a few basic axioms and definitions, with logical connections, from which hypotheses and empirical laws are deduced (Hull, 1943). Lane, in his study of Festinger's theory as applied to smokers and lung cancer, is presuming on this method, and most social scientists, at best, are doing the same.

It used to be very different. Science was supposed to be primarily inductive and not hypothetico-deductive. Hypotheses were supposed to be grasped by *observation*, not logic. The role of observation now, however, is the modest one of serving where hypotheses are *tested*, that is, merely to read a pointer on a scale, or to calculate a significant statistical difference at the conclusion of an experiment (Braithwaite, 1953).

For a number of reasons the current emphasis on being hypotheticodeductive, or explicative-deductive (Mandler & Kessen, 1959), or anything primarily deductive, is to be questioned as pertinent in socialpsychological science generally. Science is more than logic—and not in the least like geometry, as Huyghens noted three centuries ago (1678). The real interest in science is in the postulates as such, not necessarily in logical derivations from them. With the great alchemist and mysticist, Newton, we are all fascinated by deductions from systems. But reliance on Newton's theory of gravitation, great as it was, probably held back the course of physics for a hundred years (Bronowski, 1953) until Einstein could question the postulates, and so set physics on its devastating way again. Similarly for Festinger's theory—one should question the postulates, not as argument but as science, and this means by any means whatever, the hypothetico-deductive being only one way at one's disposal.

We shall put the primary interest, therefore, where it belongs—at the postulates. But there are different ways of being scientific about them. Before looking at these, however, other matters have to receive attention.

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At a basic level research on human behavior should be pursued scientifically on very few cases—in principle upon one person, and certainly one at a time. How can this be justified?

One might assume that almost anyone will serve as well as anyone else, since in important respects we are all much alike—it is often said of motivation research (MR) that just as nearly everyone has two eyes, so nearly everyone has the same basic needs. Similarly, in experimental psychology, Ebbinghaus performed all his classical experiments on memory on himself: the *processes* at issue, it could be supposed, would be much the same for almost anyone. Laboratory work in communication research has the same supposition, that *persuasion* (Hovland, Janis & Kelley, 1953), for example, can be studied *per se*, on very few persons, say ten in an experimental group and as few as 'controls.' The assumption is that all tend to be alike except for 'errors' which can be progressively reduced by averaging for a sufficient number of persons. We shall make no such assumptions, but shall regard each person as different until evidence can be given to the contrary.

But there are semantic difficulties about the terms 'general,' 'conclusion,' and 'hypothesis,' stemming from mistakes about what are synthetic and what are analytic statements. Thus, we shall distinguish between a conclusion and a general hypothesis. To say that adults in Chicago smoke 5.6 cigarettes per day on the average, is a summary of data, a conclusion. To say that smokers are dissonant is synthetic, a hypothesis: if verified, it is not a summary of data, but, instead, has 'general implications' that the same conditions will hold true elsewhere. (The same could scarcely be said about the average number of cigarettes smoked in other cities.) One has to explain, therefore, why one can make this 'general implication,' and, for this, scholars were wont to give a metaphysical reason, that there is fundamental order in nature upon which one presumes. Keynes (1921) espoused this, as did Burt (1952) in factor analysis (R-methodology).

Few scientists, however, like such metaphysics, and it is with relief that they turn, as we do, to logicians who remind us that generalizations are essentially rules which help the investigator to "find his way about in reality," by directing attention to conditions for observing instances of 'things.' Generalizations are not implications about the unity or lawfulness of nature, but for future use (Schlick, 1931). Because psychologists have not understood this they have been terribly busy trying to grasp lawfulness as conclusions, and have never thought of enunciating laws as mere rules to guide their inquiries into things. Thus one might assert an important law, which should be called Freud's law, to the effect that in conflictual situations the person may defend himself by anomalous forms of behavior (such as we variously classify as projective, rationalized, compensatory, etc.): this, certainly, is a guide to much in dynamic psychology. It is not operational, however, i.e. it is not involved in measurement, and thus has never been acknowledged as a law in the sense that the Fechner-Weber law is well accepted. But when operations are involved, as is the case in O-method application of Freud's law (Stephenson, 1953), the stature of the latter could grow. Thus, in a work to be published elsewhere (Stephenson, Intimations for self-psychology), it was important to name three such laws, Freud's just mentioned, another, Sullivan's law of me-you dynamism, and another, Rogers's law of ideal-self. These are not merely flattering designations: on the contrary, they mediate conditions of instruction for O-sorts, which provide the operations essential to a science. Moreover, in the application of these laws, as in the Fechner-Weber case, the concern can be with one person; the precise facts will usually be quite different for everyone; but they involve the same laws, not in the sense of predictability or regularity, but merely in the sense that without the laws no one would know what to look for.

This is not to say that there are no general conclusions, if by these we mean invariances or constants, as the constants g and e are invariants in the physical sciences. Density, and surface tension, are invariants in the study of physical properties, and in all such the concern is with scalar quantities only (mass, time, length). The task of isolating such invariants is quite difficult in physics, and one might anticipate no less difficulty in the human sciences. However, there are other ways of looking for invariants besides those based on physical quantities. The theory of quasi-properties (Scott-Blair, 1950-51) is an example; and factor analysis provides a most notable attempt on the part of Spearman (1927) to determine a person's central intellective factor (g) as an invariant, a pure number, for the person. The constants in Lewis Richardson's theory of foreign politics (1939) might be of this profound order. For our own part, however, it is proposed neither to emulate the experimentalists who ought to be looking for invariants, nor the very erudite who find them: the concern is with explanation of concrete behaviors, and the only important generalizations (as distinct from conclusions) we hope to use are laws and, as we shall see, abductions which serve to guide us to such explanations.

Abduction, Deduction, Induction

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There are other logic-of-science matters to look at. A solution has to be found to the problem noted at the outset, that there is chaos at the postulatory beginning and chance at the testing end of a theory. For this we propose returning to first principles of logic-of-science. At the turn of the century, the founder of pragmatism, C. S. Peirce (1931-35), distinguished between three stages in scientific enquiry-abduction, deduction, and induction. Abduction was variously described as 'retroduction,' 'presumption,' 'argument hypothesis,' the 'guessing instinct,' etc., and was concerned with the invention or creating of hypotheses. Abduction is what one does in guessing or inventing, or proposing a theory or explanation or hypothesis: it is the initial proposition to explain facts. Deduction thereupon explicates the initial proposition, deducing the necessary definitions and formal hypotheses for empirical testing. Induction is then the empirical establishment of the hypotheses. That is, there is a stage of scientific enquiry which precedes any hypothetico-deductive framework, or any such as Festinger's theory attempted, in which the emphasis is on the discovery of hypotheses, not their deduction from postulates.

Now it is true that philosophers have been quite unable to do anything with Peirce's ideas about abduction. Reichenbach (1939) decided it meant only "induction in a wider sense," as though numerous hypotheses were being tested ostensibly. Burks (1946) wondered why Peirce should wish to think that abduction was sufficiently distinctive to warrant it being called a new kind of argument in science, comparable in importance to deduction and induction. Peirce seems to have wavered in his explication of it. However, he expected abduction to have a "perfectly definite logical form"-with no bunkum about insights, dreams, or urges of the kind associated popularly with discoveries in science. But its logic must be "very little hampered by logical rules" (Peirce, 1931-35, 5, p. 188), because, otherwise, it would involve the application of knowledge already attained, and therefore could scarcely be evolving discoveries. Such logical rules as Peirce offered were of the kind that Popper (1950) would call falsification, or else were practical aphorisms, of the kind "test the simplest hypothesis first," or "test the least plausible, or the most extreme first, since its status is easiest to establish."

Reichenbach had to conclude that analysis of scientific method will always be unsatisfactory unless it is kept in close touch with actual scientific work in all its technicalities (Reichenbach, 1939, p. 191). We believe this is very sound advice: the argument for a return to more serious regard of abduction, as a first stage of enquiry, rests upon the pragmatic need to which attention has been drawn above—any honest

description of what Dr. Lane was doing, in relation to Festinger's theory, requires a rubric of the kind contemplated by Peirce as abductive. Somehow we know that interesting facts will be found: but we cannot deduce what they will be. There is chance at the postulates and again at the testing. One muddles around with pseudo-deductive frameworks, but the results are never compelling, often contradictory, and entirely of an ad hoc nature. It would be wiser to be frank, and to procede with abductive logic in mind.

There are two senses of the word abduction, however: one is a broad conception that "if ever we are to explain or understand things at all, it must be in this way." That is, one knows broadly what is to be expected, but can't say precisely what-which is what Festinger's 'theory' really amounts to. One has an explanation ready when the facts become available. Thus, the original hypotheses having been proved erroneous, Lane proceeded to look around amongst the data and found that one could explain the results by supposing that smokers "sought to justify their anxieties, and thus to support conceptions of themselves which they wished to maintain." Knowing postulate (c) of Festinger's theory, one is in a better position to know what to look for in the data -such is abductory use of a theory. But there is also abductory logic to consider, as a loose body of pragmatic rules, aphorisms, and (we venture to suggest) technique. Is it not strange that with the great growth of sophistication in logic-of-science in the last two decades no place has been found in it for technique other than an adjunct to induction, that is, as instrumental to the testing of hypotheses? But a telescope, or a microscope, or for that matter a Rorschach ink-blot, makes discoveries possible, on a first-time basis, prior to any deduction. As such, technique comes properly within the rubric of abductory methodology in Peirce's sense.

Thus, we can conclude without a doubt that most theories in psychology, such as McDougall, Allport, Freud, Hull, etc., have propounded, are essentially abductory in the broad meaning of the word. We are also sure that what Lane was practicing when he tried to be orderly in scientific respects was arbitrary—he was the better scientist when he was driven to find an excuse for the failure of his deductions, and in doing so fell back upon a facet of abductory logic, guesswork, which is completely sound in principle. It is the role of technique to take some of the guesswork out of abduction.

(To be continued)

SCIENTIFIC CREED - 1961: ABDUCTORY PRINCIPLES

WILLIAM STEPHENSON

University of Missouri

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Part I offered a criticism of the belief that human behavior is being scientifically studied by asserting a theory from which hypotheses are deduced or implied and then tested inductively (so that explanations are conclusions to deductive inferences). Instead of a few postulates, there are innumerable; and instead of one person to be tested, there are also innumerable: so that chance-like conditions exist at both ends. One asked, therefore, how science was to be possible. It seemed best to indicate how by way of a creed which would serve to suggest, perhaps, why the obvious answer is acceptable to so few. The creed began with an affirmation of faith in conclusions, generalizations, laws, and invariances. It also re-introduced abductory principles, forgotten for fifty years, to serve instead of deductive-explicative or hypotheticodeductive procedures. It asserted that it must be possible to conduct scientific enquiry on single cases.

The creed continues, with reference as before to Festinger's theory of cognitive dissonance, which was considered to be representative of current practices in the study of human behavior. It is clear, however, that there is more to say about abduction.

Abduction and Factor Analysis

The meaning of abduction first became clear to the author in relation to factor technique. This, ordinarily, is thought of as technique in the sense that its methods are of a routine, mechanical nature: much of it can be programmed for digital computers. But it is not the case for the centroid method of factor analysis (Thurstone, 1947). The equations of the centroid method can be solved in an infinite number of ways, for which reason it finds little favor amongst statisticians who value determinacy (Kendall, 1950). The factorist tries to get out of this difficulty by rotating axes in space into different positions to reach a solution that is acceptable. Thurstone looked for 'simple structure', a supposition that if the variables had 'real' and not arbitrary relationships, these would be found, even if it took months or years to find them. Thus Cattell (1948) spent something like six working years of a technician's time searching for simple structure in a study of temperament traits. We are to propose that, from the abductory point of view, the indeterminacy of the centroid method is its most important attribute.

The previous discussion of abduction noted its two-fold meaning as a matter of inference, that if facts occur they will have such-and-such an explanation (but the facts cannot be predicted or deduced, nor are

they statistical), and as a matter of methodology, that it involves pragmatic rules but also finds a place for technique as abductory (and not merely as induction in the testing of deductive inferences). The concern primarily is with discovery. A virtue can therefore be made of the centroid method's indeterminateness by rotating deliberately so as to bring unexpected but not unsuspected results to light, that is to make discoveries. The empirical search for these is not induction, but abductions in both senses of the word just noted. The scientist looks at the space-data with a broad abductory theory in mind that in it there will be facts that can be explained by the theory, as genuine, not ad hoc hypotheses. This is abduction in the broad inferential sense. But one can also apply abductory methodology or pragmatic rules to further a solution to the rotation problem, and these are legion. Thus, Cattell in the aforementioned study spent a long time looking for simple structure, but an acceptable solution was readily available in a matter of a few hours (Stephenson, 1956) along abductory lines. The bit of 'logic' was unique to the situation, as most will be.1 Thus, the infinite numper of solutions by the centroid method corresponds to the untimited horizon of abductory methodology. Or, if we return to the innumerable possibilities noted earlier for Festinger's postulates (a) (b) (c), these correspond, in the present logic, to abductory theory broadly regarded; but when subjected to factor analysis, factors provide the discoveries, the creative abductions, not in the sense of factors being found like marbles in a bag, but in the sense of factors being abduced. The logic is that if certain facts occur, they will have such-and-such an explanation. Thus Lane (1959) was able to refer the facts that nonsmokers rather than smokers shunned the TV program on lung cancer, to the need of smokers for "self-justification." One expected something of the kind, but couldn't predict what precisely. One knows, so to speak, that there are marbles in the bag, but whether large or small, glass or marble, rough or smooth, etc., etc., is quite unpredictable. Thus, Meehl (1954) in particular, and Smith (1960) more generally, seek for regularities among the diverse facts of human behavior, using statistical method as the modus operandi: but here the situation is the other way round-one finds the unique or diverse in the regularity of an abductory theory. Similarly for abductory laws: these presume regularity, but the explanation can only be given after the facts are observed. In the case of Freud's law (page 6) one expects dynamic facts to issue out of situations of conflict-but whether rationalizations, projections, condensations or the rest it is impossible to predict. The emphasis, however, in scientific work is on operations to provide the factssuch as O-sorts, leading to factors-in which case explanations are discoveries and not merely conclusions to deductive inferences.

¹ Among Cattell's variables there was a measurement of intelligence, which he withdrew from the matrix of correlations for the traits he was studying on the ground that it wasn't a temperament trait. We retained it, with the argument that it could be regarded as uncorrelated with temperament (as everyone supposes, including Cattell), and rotation began with this assumption. Such is a bit of abductory 'logic.'

Forms of Inference

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Further light on abduction can be thrown by referring to four modes of inference which Peirce (Buchler, 1950) distinguished, which, it seems, go in and out of fashion like women's clothes. They are ad hoc extensive induction, intensive induction, abduction, and general deductive inference.

Ad hoc extensive induction is typified by the hypothesis that smoking causes lung cancer. It is first shown to be true for one sample, and then for another. The more often it is tested, on ever larger numbers of cases, the more general the induction becomes. Finally, one accepts it as universally true: this ultimate induction, clearly, has merely extended an original generalization. The result is a scientific statement, often, without doubt, of considerable importance. The human behavioral sciences are studded with such inductions, all of them ad hoc. In communications theory, for example, all 32 major conclusions discussed in a paper by Schramm (1955) are of this nature. Waddington (1941) has earlier noted how the social and psychological sciences are replete with such, and adds, as well, that they are "merely things given, but not things which have been received and used and turned into something by the creative imagination." One merely tests an original proposition by extending it from general to more general. Generalizations of the kind are clearly no open road to science.

Of course it will be said that scientists have to be certain of their facts, and this cannot be denied. But it is also true that one chemist using one cigarette and one mouse, will one day demonstrate to everyone just what in smoking causes lung cancer. The method, clearly, will not be ad hoc extensive induction, but, instead, intensive. Dewey's example (1939) of intensive induction is the classical one: malaria was well defined as an illness long before the correct explanation of its genesis was understood. Some scientists thought it had bacillic origins, and others that it was parasitic. It was discovered as a parasite in the blood of malarial patients, and the cycle of parasitic life in the blood stream corresponded with the disease cycle. Another observation and deduction was necessary, however, to complete the explanation-a mosquito bite, from a mosquito which had fed on a malarial patient, was the key. Only a few patients, a few mosquitoes, and intensive inductions, were essential to the understanding. Such is a typical bit of scientific work of a Pasteur, a Fleming, a Faraday, or a Fermi. One reaches an understanding as the outcome of a chain of reasoning and observations. No one extends the result to thousands of patients before being sure about the facts because a compelling chain of inductions makes this unnecessary.

As for abduction: it is not, of course, a mental process of any kind, nor any matter of classifying according to fiat or categorization. Initially, Peirce considered it as inference, like induction, but concerned with explanation, whereas induction was descriptive—one proceeded from a sample to the whole in induction, but from the whole to an explanation or interpretation in abduction. Later, Peirce (Burks, 1946) widened abduction to include methodological as well as inferential (metascience) logic—and this is what we have done earlier in distinguishing between abductive method, and abduction as a broad theory. The two, the methodological and the metascientific, join to make a "habit of enquiry," "guessing instinct," or "presumption." The concern is with causes, not with the discovery of regularities. Laws are to help one to find the causes.

Consider, for example, Peirce's own illustration (Buchler, 1950). If I am in a foreign country and see two priests, something about them may make me believe they are Catholics. I have noticed a few attributes, a, b, c . . . about them, (such as that they are walking together, dressed alike, near Catholic churches, etc.) and I suppose than since $a, b, c \dots$ are true of them, other attributes $\dots p, q, r \dots$ are also likely to be true. These latter can be reached, however, in different ways. If I define beforehand what I shall mean by a Catholic priest, the additional characters p, q, r, will be in the definition. Thus, p might be 'devotion to rosaries'-a matter I could test. I might lay a rosary on the sidewalk in front of the two priests: if they are Catholic they will pick it up reverently; if not, they might pass it by or hang it on the sidewalk railings like an old sock. Such is deductive inference and its methods. It is involved wherever the talk in science is about validity. or about operational methodology (Bechtoldt, 1959). Abduction, however, is not of this purely analytic kind. There is a presumption instead of really different, hitherto unsuspected characteristics p, q, r ..., and these have to be reached by ways other than analytical, statistical, or hypothetico-deductive. The artist, or poet, or creative scientist says he reaches them by intuition. Certainly the great theories in science (as elsewhere) have about them some such intuitive grasp, sometimes a "somewhat cranky half-baked look." The language in which they are expressed may be "uncouth and full of newly invented jargon, feeling after the new ideas which the author cannot vet put down simply but must try to convey by implication rather than by precise definitions." (Waddington, 1941.) The abduction for the two priests is not (i) that they look like Catholics, but instead, (ii) are they? Any definition of (i) would never reach into anything new, whereas along line (ii) we are doubtful or the like, for a reason, and may learn indeed that the priests have a pretense of being Catholic, but "in their souls" are uncertain, or are brooding a devout ecstasy that far exceeds ordinary duties of a priest, or the like. One not only explains the initial facts $a, b, c \dots$ but points creatively to others.

The Logic of Discovery

The logic of discovery thus calls for a muddling-through, lying between the extremes of Baconian induction and Newtonian deduction,

and it is to this that abduction refers. Given a telescope, the German astronomer Galle saw Neptune, as Adams had predicted: but he could also observe much else, and science progresses by such instrumentation. But abduction is more than arriving accidentally or incidentally at such discoveries. The scientist knows broadly what he is looking for. Nor is abduction merely an end explanation. Thus, when the geologist notices that the rocks in Missouri teem with fossils of early marine life, he abduces that in remote geological time the sea must have covered the region. So regarded, the abduction is an afterthought, an explaining hypothesis, with which one's curiosity may rest satisfied-one has found a fact and an explanation together. Abduction is much more: it is for future use, like a law. The scientist thus goes out to meet nature with an abductory theory in mind-this is what has characterized the concepts of electricity, gravitation, etc., in physics, as well as repression or the like in dynamic psychology and cognitive dissonance in Festinger's theory.

We are near, therefore, part at least of the solution to the problem with which this creed began. The chaos at the postulatory beginning is essentially an abductory explanation in the broad inferential sense. The statistical testing at the hypotheses end can be replaced by abductory methodology. There are no doubt other pragmatic methods to serve this end, but in Q-method the indeterminacy of the centroid solution in factor analysis makes possible the discovery of factors, which have to be interpreted, that is, not as afterthoughts or as a posteriori reasoning, but as abductory—because without the broad abduction or law one wouldn't have known what to look for in the first place.

It has always been recognized that factors, in factor analysis, are first found and then interpreted. Advocates of technical factor analysis defended this on the ground that one should not allow theory to obtrude, in case bias enters (Thurstone, 1947). Critics, on the contrary, considered that this was merely being wise after the event, a trifling with science, a matter of a posteriori reasoning and poor science (Cureton, 1939). Neither standpoint is pertinent; neither touches on the central matter at issue. This is simply that factor method puts the investigator into a region, doubtingly, but with a genuine abductory theory in mind. He knows something already; but he cannot deduce consequences from postulates; nevertheless he fully expects to make discoveries, and it is technique, and the use of laws, that guides him to the discoveries. So regarded, factor method provides the first concrete exemplification of what we believe is central to Peirce's logic of abduction.

Doubt and Certainty in Science

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Neither Peirce, nor anyone else, would wish to deny importance to systematic use of theory. But scientists do not always work in an orderly manner, like butchers cutting up a carcass. Newton's hypotheses non fingo was not an idle gesture; and one should remember that he dabbled in the occult, searching for the philosopher's stone, at the time he wrote his *Principia*. The truth is that scientists are not necessarily well-ordered in their thinking, rather, they potter around in the toolshed, finding new ways to measure things more exactly or the like. As Young (1951) puts the matter:

He is continually observing, but his work is a feeling out into the dark, as it were. When pressed to say what he is doing he may present a picture of uncertainty or doubt, even of actual confusion. (Young, p. 2.)

Skinner (1950), in psychology, adds that theory isn't as necessary as it is supposed to be. Rather, the concern should be with manipulable variables, applicable to single, concrete situations—with one rat at a time, one person at a time. Measurements about average organisms, rats or men, are not enough. Variables, too, must spring from the concrete situations, as an operant response curve does in a Skinner box for a single rat at a time. Without commitment to theory, Skinner holds, one is more likely to make discoveries; moreover, as data accumulate earlier theories have usually to be discarded anyhow—theory construction is indeed the graveyard of science. All of this is part of our own creed as well.

The dearth of manipulable variables is particularly noticeable in human behavior research. Such as there are are different for every problem: there are no Geiger counters or swinging pendulums in human behavior research. Thus, in Dr. Lane's case, whether one looked at a particular TV program or not constituted the manipulable variable. This is an *ad hoc* and quite unsystematic variable. Opinion and attitude scales are systematic, but applicable only to sampling-population conditions, for averages of persons, and for *categorically* defined attributes, not for *operations* as such. Our variables in Q-studies are of course Q-sort descriptions which, besides being extremely versatile, are also highly manipulable and operational *sans* definition.

It is a fact that we have never standardized any Q-sort, or a Q-sample, or a frequency-distribution for a Q-sort, or any condition of instruction, or any procedure for solving the rotation problem of the centroid factors. This is a consequence of merely being scientific. It may look muddled and unsystematic, yet it is quite the reverse, and what is systematic in it passes without notice from anyone. Thus, a self-description is "operationally defined" as a Q-sort—which is an interesting achievement. The basic matter is not such definitions, however, but operations by persons, the Q-sorts which are correlated and factored to bring regularities to light, which in turn, define classes of self-descriptions. The fundamental data are the operations by persons, not operational definitions of self-descriptions. Moreover, no defini-

tions obtrude anywhere—neither by standardizing the Q-sample, the Q-distribution, or the Q-conditions of instruction, or the conditions under which the centroid rotational problem is solved (e.g., one does not necessarily look for "simple structure"). Yet hidden within all of this apparent lack of system and uncertainty there is a standard measurement which everyone passes by without as much as a passing thought—it is the reduction of all scores, of all Q-sorts, of all persons, under all and every condition of instruction, to pure numbers (standard scores), whose mean is 0 and standard deviation 1. A Pythagoras would have given his arms for such a remarkable simplification, which, in psychology, from the centrality of self standpoint, is as fundamental as are the units of length, time, or mass in the physical sciences. There is a paragraph in Popper's *The Open Society* (1950) which says something of the same kind: statements made in a science, he says, never depend upon the meanings of terms:

. . . even where the terms are defined, we never try to derive any information from the definition, or to base any argument upon it. This is why our terms (in physics) make so little trouble. We try to attach as little weight as possible to them . . . we do not take their 'meaning' too seriously. We are always conscious that our terms are a little vague (since we have learnt to use them only in practical applications) . . .

Similarly with regard to Q-applications: it is never necessary to talk about self-concepts, or comparable pretentious constructs, nor is the concern ever with validity, reliability, or other categorical presumptions. Storms are raised about these trivialities, but the reduction of all measurement to pure numbers—thus freeing our science of countless individual units and countless dimensions—is not even noticed or given a single glance. So the shoe is admired and the foot ignored.

Logic of Explanation

A word or two is in order about scientific explanation, since our concern is essentially with explanations rather than with regularities. We shall use laws, and no doubt discover some: but the purpose is to give explanations.

Toulmin (1953) suggests that there are three logically distinct types of explanation—a stated reason, a reported reason, and a causal explanation in a material sense. When a person says he smokes to soothe his nerves, he is giving a stated reason. If his friends say that he is always on edge unless he has a cigarette in his hands, it is a reported reason. If a scientist says that the taste of cigarettes is due to the tar and nicotine content, the concern is with material causes. Philosophers, including Toulmin, are fond of denying any scientific status to stated reasons: if a man says he feels sick, no one need believe him, and what he says can never be proved either true or false, that is,

by way of any stated reasoning. But this, unfortunately is apt to be taken too far—there are some who think of Q-sorts as in some way only stated, not reported or 'causal' in any scientific sense. Clearly, no one is suggesting that we have to believe a person's self-descriptions: but it is a simple matter to show whether or not what he says is consistent and easy to show whether other regularities are to be observed for it as well, as when what he says about himself correlates with what others say about him. With such facts as a beginning, it is possible to go further, and to give reported and 'causal' reasons for the facts.

First, however, it is important to distinguish between ad hoc and genuine causes, explanations, or hypotheses. In current jargon, a fact is explained if it is the conclusion of a valid deductive inference—only genuine hypotheses explain anything. Thus, when salt is put in water, it dissolves and salt is said to be soluble in water. Solubility is thus attributed to the salt. But this is an ad hoc explanation, not a genuine one—it tells us nothing new and nothing more than is contained in the statement that salt, put in water, dissolves. The explanation only becomes a genuine one if we can say that in solution the molecules of salt are held in suspension in Brownian movement (or the like), for reasons that can be given deductively, involving other 'primitive' tests.

Interestingly enough, factors in R-method (with which factor analysis is usually identified) have been either ad hoc, or invariant, but never genuine in conception. The topic of invariance has been mentioned already. The attempt of Spearman (1927) to prove that g. the central intellective factor, is invariant, was unsuccessful-still less is invariance reached by any other factors in R-methodology. All Rfactors, instead, like solubility of salt, are ad hoc explanations.2 Qmethod, on the contrary, does not concern itself with ad hoc explanations; it is involved in genuine explanations. Thus, if a woman gives a O-sort self-description, and then (with the same Q-sample) a O-sort description of what she thinks her husband is like and these two descriptions are alike, constituting a factor, then one seeks a 'cause' for the identity: either the woman has misunderstood the instructions, or she is so identified with her husband that one suspects either idealization or projection-and the nature of the factor itself, that is of the order of the statements of the O-sample for the factor at issue, will allow us to say what it is. The concern in every instance of a factor in O-method is with genuine hypotheses, genuine explanations. Each leads directly to additional 'primitive' tests; each is a conclusion to

² One finds that mental tests involving numbers (arithmetic) are clustered in factor space, i.e., they can be operationally classified as alike—and one calls the factor n, or number-factor. Similarly v is the factor for verbal tests of intelligence. To so designate factors is clearly only of taxonomic interest—no genuine hypotheses are at issue. It would have been very different if tests of number, and tests of color-blindness, happened to cluster as one factor: in that event the factor could scarcely have been called either number or color, and one's curiosity would have been whetted to find a genuine explanation for such an interesting fact. There is not a single convincing fact of the kind in all R-method, and therefore scarcely a genuine hypothesis anywhere at issue. This not true of Spearman's own work, which Thurstone and others quite misunderstood in this respect: Spearman looked for genuine explanations (eduction), for wholly dissimilar tests.

valid deductive possibilities (by way of the structure, etc., of the Q-sample statements).

It remains only to say that in view of the additional tests possible for each Q-factor there is no logic-of-science difference between what the physicist would call a 'causal' explanation of a material kind, and what a psychologist calls a 'causal' explanation of a psychological kind. The chemist says that it was arsenic in the lemonade that poisoned the picnic-makers: the psychologist, that it is identification that has caused a woman to enjoy a radio day-time serial so fully. This is more than a reported reason (in Toulmin's sense): it is 'causal' in so far as there are other ways to test the hypothesis of identification, by way of more 'primitive' tests.

(To be continued)

SCIENTIFIC CREED-1961: THE CENTRALITY OF SELF

WILLIAM STEPHENSON

University of Missouri

The Problem

The replacing of chance by order at the testing end of a theory is achieved in part by abductory logic, as discussed in the preceding sections of this creed, but more generally by a radical change in methodology, away from the sampling-methodology which Smith (1960) would have us accept as a "definite type of explanation" to a methodology based on the "single case."

The present author (1935) has long stood for the acceptance of a centrality-of-self methodology, that is, for basing psychological mensuration upon the single case, rigorously and without equivocation. Because of the dominance of sampling doctrine, upheld as it is by all the statisticians and professors in the academies, the case for the single case has barely made a dent in current psychology or in social science. Yet it makes science possible, in the place of scientism. There are, it is true, a few signs of disquiet: Selvin (1957) suggests that statistical tests of significance in survey research are ineffectual; Williams (1959) is very critical of survey techniques, doubting their validity as well as their assumption of equivalence of responses from different individuals-he does not say so, but a comparative methodology (such as was introduced earlier (Stephenson, 1953) could assauge his doubts. On the extreme psychological side, existentialists (May, Angel & Ellenberger, 1958) are so sure that psychologists are missing everything important that they reject scientific methods lock-stock-and-barrel-so throwing out the baby with the bathwater, for it is not difficult to make existentialist doctrine toe the line with legitimate science. There is also, in this country, some interest in self-psychology: but it is based on sampling methodology, for example in the studies edited by Atkinson (1958) under the title Motives in Fantasy, Action, and Society.

From our standpoint the threads of a non-sampling standpoint go back to Soren Kierkegaard's famous Concluding Unscientific Postscript (1941). Much later, James Ward, in his forgotten Psychological Principles (1919), picked up the threads, but instead of throwing away scientific method as the followers of Kierkegaard have since done, he recommended a centrality-of-self methodology to match the obvious truth that persons, as such, exist. Ward could not say what the methodology would be: he reasoned, however, that it would lead to comparative systems as distinct from differential or ad hoc ones, and to the proper use of scientific classification as distinct from nominal

categorizations. Twenty-five years ago the solution to Ward's problem was found (Stephenson, 1935), but, as was said earlier, few were prepared for it. Current attitudes are against it amongst the professors, as they were in Kierkegaard's day, in nomine scientia. Yet the methodology reaches into self-psychology, into existential doctrine, into psychoanalytic theory, into personality theories generally, into motivation research (MR), into a general theory of mass communication, and into much else, including a mensuration for the measurement of public opinions, tastes and attitudes typologically, besides the various areas covered in an earlier work (Stephenson, 1953): and it is all based on the following very simple creed.

Beginnings

The section on 'generality' (above, page 5) introduced the problem of research on the single case. Ebbinghaus (1913) researched entirely upon himself, and his work has long been regarded as a landmark in the application of the theory of errors of observation to the examination of higher mental processes. Ebbinghaus discusses the law of error in detail, made extensive use of 'probable error' calculations, and worked with an exactness not excelled at the time in many major experiments in physics (Walker, 1929). It is true that no two persons are alike any more than two lumps of iron ore are alike: but for analytic purposes one lump is as suitable as any other and one person as suitable as any other.

Science is possible for any such person by being scientific, that is by being properly selective, abstractive, and devoted to 'operations' leading to classifications, as Ebbinghaus was. The fundamental task in a science is rarely, surely, to provide meticulous descriptions of phenomena, as some suppose (see, for example, Williams, 1959), but, rather, to select from it what it likely to prove important. There is missing, throughout psychology and the social sciences, any real awareness of what it is to be selective in the scientific sense. No one has hit upon the molecules of self-psychology (or any other), comparable to the 'bits' of information theory. Instead of being selective, psychologists have chosen nominal categories ('processes,' etc.) for research, such as persuasion (Hovland), attitudes (Sherif, 1947), etc., or else non-psychological elements such as reflexes (Behaviorism).

We propose instead that the basic elements in all self-psychology (and all psychology based upon it) are a person's self-referent statements or notions, made in relation to interactional situations (Stephenson, 1953). The latter always involve the person, X, the social 'conditions,' Y, and the mediating circumstances, Z—whether the person is on a psychiatrist's couch, or being studied for his motives in purchasing soap (Wiebe, 1951), or for his 'being' and 'non-being' in an existentialist's consulting room. Examples of self-referent statements were given in our earlier work (Stephenson, 1953), where Freud's early case,

Dora, was considered methodologically. In a study (Stephenson) of Khrushchev's visit to this country a person X, in relation to the mass media Y, expresses himself about Z, Mr. Khrushchev. A self-referent statement would be of the kind: "I saw him on TV, and I still wouldn't believe a word he says."

Such statements, taken out of whatever XYZ context is chosen for study, can be categorized (nominally) as self-involving, self-denying, self-reorganizing, self-activating, self-reflective, self-justifying, and the rest. But such categories are not given by operations and therefore are not of direct concern to science. Nor is everything a person says in a particular XYZ context necessarily self-referent, although it may seem difficult to divorce him, qua self, from any of his discourse or remarks. Much may have at most a remote or tenuous connection with himself, as when a man may say: "My wife, now, she never bothers about Khrushchev." Self-referent statements, so considered, are the ubiquitous elements of self-psychology, and constitute the basic selection of our science.

One not only selects, but is abstractive. In the theory of gases, molecules were represented as minute billiard balls about which expressions of motion could be written and the theory generated. Similarly, a person can re-present aspects of himself as Q-sorts, which he performs under various conditions of instruction. The aspects of self are abstractions: yet they bear a certain homologous relation to the real things, as minute billiard balls do to molecules. The abstractions, moreover, are operations, operant by the person himself, as truly and as significantly as any of Skinner's pigeons is operant.

Furthermore, laws are used, as "directions, rules of procedure enabling the investigator to find his way about in reality, to discover true propositions, to expect with assurance particular events" (Schlick, 1931), to indicate what conditions of instruction the scientist gives to the subject X. He is not just told to give a description of himself, as a self-concept: but, by way of, say, Sullivan's law of me-you dynamisms he is called upon to describe in one Q-sort himself, and in another (with the same Q-sample) what he thinks his wife thinks of him. Or, in using Freud's law, one invites Dora to describe herself, and later, what she thinks Frau K is like (who, for Dora, happened to be an object of homosexual regard). Similarly for other laws indicating conditions of instruction.

Further abstractions occur when the different Q-sorts for X are correlated and factored. Factors are merely classes of Q-sorts, an operational basis of classification, of the kind upon which science flourishes and the basis of comparative methodology. The factors are expressions of regularities in the lawful sense—as was said earlier, the precise facts

¹ The reference is to Freud's famous "A Fragment from a Case of Hysteria," Collected Papers, III, p. 13.

in applications of laws such as Sullivan's or Freud's will usually be quite different for everyone, with different Q-samples, etc., but the *factors* can be the same for everyone, showing the same regularities, requiring the same explanations.

In all the above there is no question at all of collecting any 'standard' Q-sample; nor is it assumed that the statements mean the same to everyone; nor is any question of validity or reliability 'in general' at issue. The concern is with concrete interbehavior (Kantor, 1933): yet the scientific work can proceed, as it did for Ebbinghaus, with the delights of the law of error, of small sample theory, of factor analysis, of variance analysis, and the rest to help one along. Now-adays, too, there are high-speed digital computers to make what used to be tedious a joy.

The Methodological Choice Point

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Our concern is always with a person. If X is asked to describe himself in relation to an XYZ interaction, he can express his belief, his opinion, his idea, his thoughts, his attitude, his notions. In doing so he will use self-referent statements, and, in our science, will do the same abstractly as Q-sorts. He can of course as readily describe what he believes to be the beliefs, attitudes, opinions, etc., of others—his wife, friends, the "TV industry," "the government," or even what he supposes most people in the world believe.

Now there are two ways of proceeding vis-a-vis these descriptions. One consists of overlooking that they are self-descriptions, and to proceed, instead, to examine the supposed attitudes, opinions, beliefs, etc., as categorical matters. The methodological problem for the categorical presumption was solved by Stern (1921) and others, using sampling theory for populations of persons varying with respect to the attitudes, opinions, etc., and all experimental methodology (on persuasion, thinking and the like) is similarly based. The operations concern the measurement of 'facts,' as Kierkegaard (1941) and Ward (1919) long ago indicated, and Sartre (1954) has done more recently. Psychology is replete with its Thurstone scales, its Stanford Communication Research Scales, and the like, all providing 'facts' but no explanations.

The other way is to remember that the primary data are self-descriptions, and not opinions, attitudes, etc. The self-descriptions are characterized by centrality-of-the-subject. One should ponder the difference, which is of the same order of things as the physicist's when he was enjoined to observe that he could not assume instantaneity, but had to remember it took time to measure it. Similarly here, the primary scientific operations are self-descriptions, and not supposed attitudes, opinions, etc., whatever the conditions of instruction implied or stated.

The centrality-of-self standpoint has of course had its advocates, for example G. W. Allport (1955): but there had been no objective,

operational solution to the methodology it requires prior to Q-method. Embroiled in it is the distinction between sampling ('viewers') and comparative ('persons') methods; also nomothesis (as 'facts,' with general implications) versus ideographic descriptions and explanations. Fundamentally, as in Ebbinghaus's case, it makes possible the explanation of human behavior for any single case. It solves a number of outstanding problems, providing, for example, an empirical solution for Max Weber's 'ideal types,' or anyone else's, such as Riesman's typology (1953). It makes possible all that was said of it earlier, for self-psychology, existentialism, psychoanalytic theory and the rest.

It is of course not denied that work of much practical or technical importance has to be pursued along sampling lines, with individuals as experimental subjects, 'viewers,' items of populations, audiences or the like. But this is never pure science, if by this is meant attending with due care to what the basic operations are that are involved in interactions: all general and sampling-methodology ignores the self it everywhere assumes, and by this alone is technological at best, and scientism at worst. For anyone to say that the centrality-of-self and the sampling methodologies are merely two sides of the same coin is surely to exercise an old attitude, a rationalism such as Kierkegaard had to protest in his day, and James Ward in his—an attitude difficult for the old to abandon since so much has been vested in it.

Fin de Siècle

Sampling methodology belongs indeed to the nineteenth century, made by the genius of Quetelet (Walker, 1929) into an imperious dominance against which, it seems, a few can only tilt quixotically. We recall Smith's conclusion, however, that an era of system-building has ended, leaving behind fact-finding and hypothesis-testing, supported by sampling at the testing. A man with a tooth-ache, visiting a city, may be going to a dentist-or he may not. We cannot predict, whatever they may do by careful observation, what the man will do (Smith, 1960). But out of a hundred men with tooth-aches a statisticallysignificant number will be on their way to dentists-of this we may be sure. This epitomises the methodology against which we tilt. The centrality-of-self standpoint studies the man by asking him what he is doing or where he is going. There is no need to believe him, since the necessary operations (of Q-method) tell their own story. Thus the problem of chance at the testing end of theory is resolved-one no longer works on a hundred men to prove a point, but basically on only one.

From this standpoint one proceeds to comparative methodologies, with operations to define the classes, not classes to define the operations.

Thus, to revert finally to Dr. Lane's problem—given *one* smoker of filter-tip cigarettes (X), a Q-set of slogans about smoking such as fill national advertisements ('a thinking man's cigarette,' 'air-softens every

puff') to represent the social mechanisms (Y), and several conditions of instruction with reference to smoking habits (Z), it is a simple matter for X to tell us what he feels about smoking. The factors, when found, can be explained abductively as cognitive dissonance. The secret consists of giving appropriate conditions of instruction on the basis of known laws—such as Freud's, Sullivan's, and Rogers' to which reference has been made.

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Or, if one proceeds to a comparative methodology, one considers smokers, some of regular and some of filter-tip cigarettes, some reformed non-smokers, and some who have never smoked at all—it may be enough to begin with five of each.² The same slogans from current advertisements could be used as a Q-sample, with one condition of instruction, the same for all the men. The factors are the foundation of a comparative study of smoker's cognitive dissonance. It happened, in our application, that the filter-tip smokers were most defensive, whereas smokers of regular cigarettes rather enjoyed their smoking, as connoisseurs like wine.

There are few problems currently approached by sampling-methodology (which assumes that individuals are alike except for 'errors' or 'individual differences') that cannot be dealt with from the centrality-of-self standpoint (which assumes that everyone is different until evidence to the contrary is forthcoming). The creed thus ends. It ends with a hope, but little faith, that the end of the nineteenth century 'error' dogma is not far off.

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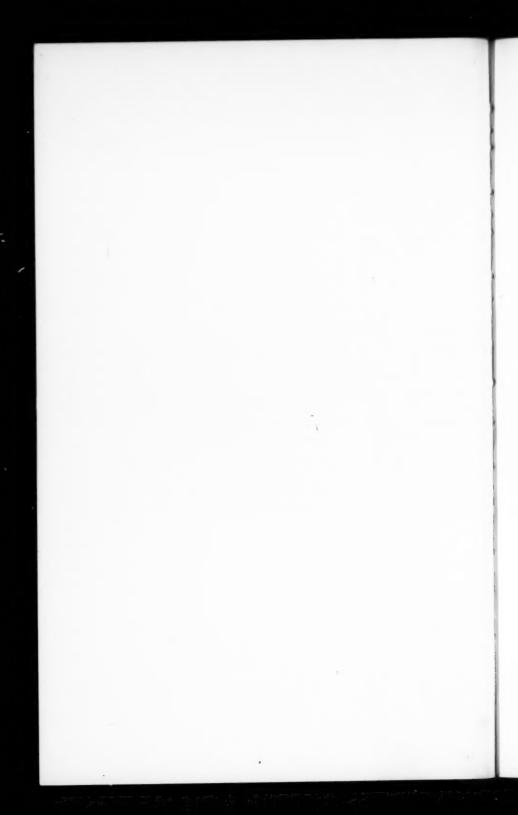
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² These classes are of nominal significance only, and are not born of operations by the subjects: only after the data are factored, as in Q-method, does one reach into scientific classification.

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A METHOD FOR STUDYING DEPTH PERCEPTION IN INFANTS UNDER SIX MONTHS OF AGE¹

ROBERT L. FANTZ

Research Dept., St. Vincent Charity Hospital, Cleveland, Ohio, and Western Reserve University

In the everlasting controversy between nativists and empiricists on the role of innate factors versus learning in the development of various behavioral functions, the battle has raged most fiercely, perhaps, on the ground of visual space perception. Even so, experimental evidence on the origin of spatial vision has been almost non-existent until recently, due to the difficulty of testing during the earliest period of development.

The testing difficulties have been overcome with infant animals in some studies (Gibson & Walk, 1960; Hess, 1956; Fantz, 1958a) by taking advantage of responses which mature early and do not require training. But human infants show few coordinated responses to the visual environment which can serve as response indicators during the early months of life. An exception is the orientation of the eyes and head to visual stimuli, which is prominent in the behavior of the young infant. Most of our information on early vision comes from observing eye movements and fixations. However, this information concerns such features as light intensity, color, and movement (Pratt, 1946).

We have recently shown that it is also feasible to use the visual orientation response to test the perception of simulus configurations. Fairly acute pattern vision was demonstrated in young infants by recording the amount of fixation of objects differing in size or type of pattern (Fantz, 1958b; Fantz, 1959). The aim of the present study is to use the same method to study the early development of depth perception.

METHOD

The basic testing procedure was simply to expose a solid object and a comparable flat object to the infant for a series of 20-second periods and to record the length of visual response to each object. The objects were a sphere and a disc, both 6 inches in diameter and painted a non-glossy white color.

The testing was done in a chamber two feet square and two feet high. The inside was blue to give a contrasting background for the objects attached to the ceiling of the chamber.

¹ This paper was read at the 1960 convention of the American Psychological Association. The research was supported by Grant M-2497 from the National Institute of Mental Health, U. S. Public Health Service. It was carried out with the cooperation of the DePaul Infant Home.

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The infant was placed face up in a small hammock-type crib which was pushed inside the chamber so that his head was one foot below the objects. The objects were on either side of the infant and one foot apart. They were hidden between exposures by window shades drawn horizontally across the middle of the chamber.

The subjects were observed from above the chamber through a quarter-inch hole in the center of the ceiling. The direction of gaze was revealed by tiny reflections of the objects which were clearly visible on the surface of the infant's eye. The criterion of fixation was the superposition of one of these reflections over the pupil. If the right reflection was over the pupil, for example, the object on the right side was being fixated.

The response time was recorded on two electric timers which were activated by pressing spring-lever switches placed conveniently on top of the chamber.

The subjects were 52 healthy infants ranging from one to six months of age with a median age of 15 weeks. Each was tested under eight conditions, including the presence or absence of three possible depth cues. Binocular cues were varied by testing either with both eyes open or with the left eye covered. Surface texture was varied by using two pairs of objects—one pair with smooth surfaces and the other with a pronounced texture produced by a granular material mixed in white paint and spread evenly over the surface. Brightness gradients were varied by using either direct lighting from a single 75-watt reflector floodlight directed obliquely at the objects, or rela-

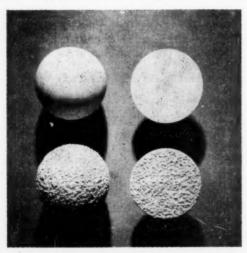


FIGURE 1. The smooth and textured pairs of stimulus objects used in the main test, as they appeared under the direct light condition with brightness and texture gradients on the spheres.

tively diffuse lighting from four 30-watt reflector bulbs placed at the corners of the chamber. Figure 1 shows the smooth and textured objects as they appeared under direct lighting.

Four binocular tests were given, including two with textured objects, either with direct or diffuse lighting; and two with smooth objects, with direct or diffuse lighting. The same four conditions were used in monocular tests. The sequence of testing under the four conditions was balanced among the subjects. When the infants cooperated sufficiently, the testing was all done in one session; in most cases, binocular and monocular tests were given on different days, with the order varied among the subjects. The test for each condition included two 20-second exposure periods with reversed positions of sphere and circle to control position effects.

RESULTS

The time scores, when totalled for all eight tests of an infant, indicated a greater response to the sphere than to the circle. The difference was significant at the .05 level according to a two-tailed Wilcoxon T test of ranked differences between sphere and circle response time.

In order to determine the stimulus basis of this discrimination, the per cent of response to the sphere was compared for the eight conditions. A significant (.05) effect of test condition was shown by a Friedman analysis of variance by ranks. Considering the conditions separately, the two tests using textured objects under direct lighting, binocular and monocular, each showed an interest in the sphere which was significant at the .01 level, while the other six conditions showed little, if any, differential response. Thus the infants differentiated solid and flat objects only with a maximal difference between them due to texture and brightness gradients on the sphere.

The overall response to the sphere showed little correlation with age. Considering binocular and monocular tests separately an interesting effect of age appears. In the binocular test with textured objects under direct light, infants under three months showed little differential (9 infants responded more to the sphere, 7 favored the circle); while the infants over three months showed more interest in the sphere (21 for sphere, 11 for circle). A different result was obtained in the monocular tests of the same subjects.² Under three months, a clear differential was shown (10 for sphere, 2 for circle); over three months, the sphere was still the favored object but less consistently (19 for sphere, 11 for circle). This result suggests the possibility that the use of both eyes

² The subject totals do not agree for several reasons. The second testing session could not be given in some cases due to the subject being unavailable; this decreased both the binocular and monocular totals. The monocular totals were further decreased by the fact that some infants were disturbed by the eye patch and could not be tested monocularly.

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interferes with vision in the early months before the development of good binocular coordination, while binocular vision improves visual performance later on.

A repeat test was given to 25 of the older infants (median age, 18 weeks) to verify the results with new stimulus objects. Only textured objects were used, thus reducing the number of conditions to four. One change was made to give a further control of stimulus conditions. Previously, the circle was placed level with the mid-section of the sphere. This equated the retinal size and the amount of light reflected from the two objects, but it of necessity brought the sphere closer to the infant than the circle, giving a larger texture and larger cast shadow as a result. The new objects were equidistant from the infant at their nearest points. Size of texture and shadow were controlled with this arrangement although the circle was now brighter and larger than the sphere.

The results of the repeat test were, if anything, more consistent than those of the main study. Preferential interest in the sphere was again shown only under direct lighting. In the binocular tests 18 infants favored the sphere compared to 6 for the circle; this was significant at the .01 level based on the Wilcoxon T test. In the monocular tests, 16 favored the sphere compared to 8 for the circle; this was significant at the .05 level.

DISCUSSION

The results can be interpreted in several ways. First we can take the viewpoint of a traditional discrimination experiment in which, for example, an animal is trained to approach a solid rather than a flat object and is then given critical tests under various stimulus conditions; or a human subject is instructed to tell which of two objects is solid with various cues available, to determine the relative ease of discrimination. From this viewpoint, the present results give evidence that infants under six months discriminate solidity through differential texture gradients and brightness gradients, with binocular cues not essential but perhaps of some aid after the first few months.

A different interpretation can be made if we emphasize the nature of the present method. The infant does not select the solid object in order to be rewarded or to carry out certain instructions and thus please the experimenter; he looks at whatever attracts his attention at the moment, with no ulterior motives. From this viewpoint, the infant's attention is directed to the sphere by the particular stimulus configuration resulting from texture and brightness gradients on a curved surface; he is showing a pattern preference rather than depth discrimination.

A more fruitful interpretation emerges from the integration of these two viewpoints. Since it has been shown that complex patterns elicit a strong visual interest in young infants (Berlyne, 1958; Fantz, 1958b), it is reasonable to suppose that the striking pattern visible on the textured sphere under direct lighting (see Figure 1) was an important factor in producing the differential response. On the other hand, it is also true that objects in general are textured and that direct lighting is the natural condition, so that solid objects outside of the laboratory can be and are recognized by the pattern of light and dark areas thus produced on their surfaces. Differential patterning is an important stimulus variable for the perception of solidity. Consequently an infant's selective interest in a solid object is relevant to depth perception even when based on patterned stimulation. Whether the difference between the objects is perceived by the infant as "depth" or "pattern" is unknowable and irrelevant.

The spontaneous motivation for the differential response of the infants, rather than being a handicap in the interpretation of the results, may actually provide a missing link in the development of depth perception. To illustrate this possibility, let us assume there is an innate interest in the type of pattern possessed by solid objects under natural conditions. This would have the effect of bringing the infant into frequent visual contact with the solid objects surrounding him—an adaptive effect since solid objects are often important for behavior. The repeated experience with solid objects would be expected to result in the association between the originally-prepotent feature and other features comprising solidity. Thus an innate interest in some patterned aspect of solid objects, representing a primitive type of depth perception, may at the same time provide the opportunity for the further elaboration of depth perception through visual learning. Whether some such process actually occurs is a question for future research.

CONCLUSIONS

The study of depth perception by observing the spontaneous visual interests of infants in solid objects has a twofold significance:

First, it gives a test of the discrimination of solidity under various stimulus conditions which is applicable during the critical initial phase of development. The present finding is that infants between one and six months of age can differentiate between sphere and circle and do so primarily through perceiving the pattern of texture and brightness gradients on the sphere.

Second, this testing method reveals the operation of what could prove to be an essential innate factor in the genesis of space perception—an active factor to supplement the inherent receptive capacity of the visual system. This factor is the spontaneous visual selection of, and attention to, features of the environment which are important for perception of the spatial world and for later adaptive behavior in it.

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There is cause for hope that through the further application of this simple and direct experimental approach, the controversy between nativists and empiricists on the development of space perception may not last for ever.

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THEORETICAL IMPLICATIONS OF SENSORY DEPRIVATION

MALCOLM H. ROBERTSON

University of Florida

Theoretical formulations spawned by the sensory deprivation studies have won a quick and apparently uncontested place in the thinking and writing of the psychological world. Such an immediate cordial reception is not altogether unexpected for this is the kind of psychological topic that comes along once in a blue moon and has a little something in it for everybody.

First, there is the dramatic nature of some of the behavioral phenomena of SD as well as the impact that some of the experimental results have had on consideration of other issues fundamental to the study of behavior under stress. Second, the SD concept has historical as well as functional ties to such first rate psychological topics as the autokinetic phenomenon, selective elimination of environmental cues in learning experiments, and stimulus satiation studies. Third, along with such experiments as those on the tracking and monitoring of signals and the activation or arousal concept, SD bears witness to the Zeitgeist which might be described as a vigorous and enthusiastic support for the investigation of the stimulus as an important source of behavioral control. However, it is principally to the first point that the comments of this paper are directed.

PREOCCUPATION AND SUGGESTIBILITY

To begin with, the assumption is made that an individual's response represents an interaction of internal stimuli (thoughts, feelings, images, memories) and external stimuli (people, objects, situations) in which one aspect of the interaction is a reciprocal selectivity. That is, both internal and external stimuli on which the interaction is based are samples from large populations of stimuli. At any specific moment, the particular pattern of thoughts and feelings which are dominant in awareness predisposes the individual to perceive or attend to certain external stimuli and not to others. Similarly, the occurrence of particular external stimuli acts as a selective factor in eliciting or emphasizing certain thoughts or feelings.

With a marked reduction of external stimuli, it is reasonable to assume that the interaction of external and internal stimuli is weighted considerably on the side of the latter. In extreme sensory deprivation, there is little or no interaction between internal and external stimuli and an individual response is probably determined entirely by various internal stimuli. With little or no external stimuli, not only is the indi-

vidual's reaction based mostly on internal stimuli but the selective suppressing effect of external stimuli is lost, resulting in a sudden crowding of consciousness with material that is ordinarily unconscious. In such a case, a person may seem to compensate for the absence of external stimulation by projecting thoughts, feelings, images, which may then be reacted to as though they originated outside the person.

Now, behavior under sensory deprivation may be viewed as a two step process. This two step process can be conceptualized in terms of two interlocking concepts, preoccupation and suggestibility, which give a sharper picture of how a person reacts under SD. For example, preoccupation would seem to be an inevitable consequence of SD. That is, with the elimination of all or at least the greater part of external stimulation (whether this be an over-all decrease, or just a reduction in patterning, in sensory input), it is reasonable to expect a considerable narrowing of the individual's perceptual and ideational reactions. In brief, the individual would focus more and more on less and less. From the preoccupation, a state of suggestibility would develop in the following way. First, preoccupation implies that something looms large in the individual's awareness. It looms large in awareness and preempts his attention because in the SD state there is little or nothing else to which he can attend, and consequently there is an excessive preoccupation in what little stimulation remains (Lilly, 1956; Scott, T., et. al., 1959.)

Second, this residual stimulation which dominates awareness is detached from any background or context that would provide structure and limit meaning. In the absence of such structure, the threshold for projection of thoughts and feelings not ordinarily in conscious awareness would be lowered, and the individual would then react more freely and uncritically to the residual sensory input.

In the next section, the usefulness of the two concepts of preoccupation and suggestibility is further examined in terms of the explanatory value they have for some familiar psychological phenomena.

SOME APPLICATIONS

Prior to falling asleep, the dark quiet room and the resting position of the body reduces sensory input to the point where the individual becomes acutely aware of any weak external stimuli. Thus, stimuli which he would ordinarily be only minimally aware of now loom large. Furthermore, these residual stimuli are perceived more or less in isolation from any perceived background or context. Consequently, the range of possible meanings that could be given to such stimuli is greatly increased. Such residual external stimulation can be used to facilitate or delay sleep depending upon the pattern of ideational stimuli (thoughts and feelings dominating awareness) with which it interacts. During actual sleep, which would seem to be a greater degree

of sensory deprivation, the importance of internal stimuli is seen clearly. For example, when subjects in sleep experiments are stimulated in different ways from an external source, their reports of such stimulation indicate considerable distortion and misinterpretation.

Hypnosis is another phenomenon that lends itself to the above type of analysis. A partial SD state occurs when the subject's field of attention is sharply curtailed by such conditions as diminished illumination, reduced background noise, and uninterrupted looking at and listening to the hypnotist. The hypnotist fosters a state of preoccupation by directing the subject to think only about what he is then perceiving. The state of suggestibility derives from the fact that the relatively isolated pattern of visual and auditory stimuli interacting with the thoughts and feelings associated with these stimuli preempts awareness. The suggestibility is shown by the subject's heightened responsiveness to the hypnotist insofar as he reacts freely and uncritically to the latter's suggestions.

The autokinetic phenomenon provides another interesting illustration of the role of preoccupation and suggestibility in the behavior of those undergoing some form of SD. The deprivation is produced primarily by the reduction of visual stimulation to a stationary pinpoint source of light of constant intensity and, secondarily, by having the subject maintain a fixed position and by eliminating background noise as much as possible. Preoccupation is brought about by the marked diminution of sensory input as the subject concentrates on the little stimulation remaining, i.e., the light.

Suggestibility as manifested in the subject's perception of the light moving, and even writing certain words or phrases (Rechtschaffen and Mednick, 1955), generally follows the line of reasoning for the previously mentioned phenomena. That is, the subject becomes markedly responsive to this one item of external stimulation, the light, by virtue of its domination of his field of awareness. More importantly, the light is experienced in the absence of those background or contextual cues which ordinarily provide meaningful structure and limits for the perception of such a stimulus. The subject then becomes quite susceptible to illusory experiences as indicated by his projections.

A current but not exactly traditional psychological issue to which the preceding analysis might be extended is the subject of brainwashing. Despite variations in techniques as well as circumstances under which it is practiced, a sufficient communality of results exists to conceptualize it as SD phenomenon (Hebb, 1957; Solomon *et al.*, 1957).

Thus, the physical and psychological confinement created by the minimal external stimulation and the limited sharply circumscribed verbal interaction produces a severe constriction of awareness. This severe constriction of awareness compels the subject into a preoccupied state of mind. The effect of this preoccupied state of mind is evident insofar as that which remains in awareness assumes enormous significance. More importantly, however, that which remains in the individual's field of awareness is in a sense isolated from customary background or contextual cues which serve to define and limit its meaning and significance. Suggestibility is then manifest in the individual's lowered threshold for projection of thoughts and feelings not ordinarily in conscious awareness.

Finally, creative activity can be construed in terms of a voluntary modified sensory deprivation. What is created is often the outcome of a period of intense preoccupation with some idea or feeling or external stimulus. The intense preoccupation is possible only because the individual has been able to eliminate distracting internal and/or external stimuli. This preoccupied state then paves the way for the suggestibility which may be the really crucial factor in being original or creative.

Previously it was stated that suggestibility occurs because something looms large in the individual's awareness and is reacted to in relative isolation from any particular context or structure. In a sense, this means that one's thinking, feeling, perceiving are loosened sufficiently that a particular phenomenon can be experienced in many different ways. One example might be that certain ideas, images, feelings which are related to the creative task but which have been below the level of conscious awareness can be resuscitated and utilized in a constructive manner. To restate the point, creativity or originality may be thought of as a deviation from the usual or the familiar, perceiving the data in a novel way, making unexpected associations, a break with conventional conceptualizations, all of which would seem to presuppose some degree of suggestibility.

ABNORMAL BEHAVIOR

The preceding discussion suggests that through the concepts of preoccupation and suggestibility important human experiences can be conceptualized as sensory deprivation phenomena. At the same time, it is in the application to abnormal behavior that the explanatory value of these concepts is fully realized.

Certainly the range of symptoms produced in the SD state, either as part of an experiment or as an unplanned involuntary experience, is very nearly as great as is found in the organically and/or functionally determined disorders. There is the irritability, the childishness, the rigidity, anxiety, bizarre fantasies, flickering delusions, transient hallucinatory experiences, and motivational deficits (Hebb, 1958; Wheaton, 1959).

However, the unusual behavioral effects which characterize the SD state probably depend not so much on the objectively determined amount of deprivation per se as they do on the size of the discrepancy between the deprivation and the customary level of stimulation for an individual. For example, a given amount of deprivation undoubtedly produces a greater behavioral effect in the case of a cyclothymic personality than in a schizoid type of personality.

To view organically and/or psychogenically determined abnormal behavior within the framework of SD, it is necessary, as was so in the discussion of creative activity, to stress the self-imposed and partial nature of the process. Thus, organic impairment can produce partial deprivation through interfering with the individual's perception of external stimulation, or with certain response patterns which would eliminate some important response-produced stimuli. However, a partial deprivation might be produced in a more indirect manner. Those with an organic impairment might adopt an avoidant or withdrawing mode of behavior as a means of eliminating the sudden or unexpected in their environment, thereby avoiding anxiety due to real or imagined inability to cope with certain situations. Similarly, in the case of disorders of psychogenic origin, a partial type of deprivation might be initiated if a severe problem distracts the person from much of the customary external stimulation. Or, due to the nature of the particular problem, the individual might be motivated to withdraw from certain types of anxiety-producing stimuli.

A detailed analysis of the role of the key concepts preoccupation and suggestibility will make clearer the application of SD to abnormal behavior. As stated above, a partial SD state could be initiated in one of two principal ways. In the first way, a particular problem would distract the individual from what normally stimulates him and he would become more and more preoccupied with the problem to the exclusion of everything else. His field of awareness would tend more and more to be limited to those external and internal stimuli related to the object of his preoccupation. The preoccupation would be intensified by the arousal of certain associations which ordinarily would be below the threshold of consciousness. As described above, external stimulation in addition to having an activating function also has a suppressing or inhibiting effect on ideational stimuli. At this point, suggestibility comes into play to complete the sequence of events.

Suggestibility would become magnified first because external stimulation is greatly diminished and circumscribed and because so many ideational stimuli are condensed into a pattern of thoughts and feelings related to the problem. Secondly, the circumscribing of external stimulation and the telescoping of thoughts and feelings not only intensifies and sharpens that which preoccupies him but it also isolates or separates it from the customary background or context of stimuli.

This background or context of stimuli through its defining and limiting qualities presumably furnishes the perspective and sense of proportion that prevents the person from reacting too freely and uncritically.

The increase in suggestibility, as the problem loomed larger and larger and as it came to be viewed in a more and more isolated or detached form, would become evident in the individual's hypersensitivity to the problem, in his loss of proper perspective on it, in his uncritical acceptance of many associative reactions, and in his tendency to project some normally unconscious thoughts and feelings.

For example, a person might be overly concerned with the reactions of people to him. In a sense, he would be so distracted by this concern that his attention would focus on it to the virtual exclusion of other sources of stimulation. In such a case, he would become preoccupied with the conversations of people nearby and the pattern of ideational stimuli aroused by such conversation. Again, because this concern would occupy such an important part of his immediate experience, his reactions would be based and controlled to an unusual degree by it. Furthermore, because of the virtual insulation of this concern from other sources of stimulation, the effects of suggestibility would come into play and the individual would become susceptible to over-reactions, distortions, and misinterpretations.

The other principal way in which a partial SD state could be initiated is as follows. The individual might begin by avoiding anxiety arousing stimuli. If the anxiety were sufficiently great and/or the stimuli were sufficiently general or common the avoidance would be easily extended, so that the individual would in a sense withdraw from or avoid most of the stimuli with which he ordinarily came in contact. A state of preoccupation would then follow in which the individual would focus on a small circumscribed segment of external stimulation and the pattern of ideational stimulation elicited by this. Furthermore, the suppressive function of external stimulation would be weakened correspondingly and the person would become aware of hitherto unconscious thoughts and feelings. At this point, preoccupation would usher in a state of suggestibility. In other words the same restriction of stimuli that produced the preoccupation would also create suggestibility by magnifying the little that remained in awareness and isolating it from reality considerations.

For example, an individual might begin by withdrawing from one class of stimulation, e.g., heterosexual contact. The avoidance might be extended as more and more stimuli were found to arouse anxiety relating to heterosexual contact. Soon a considerable restriction of contact with the environment would be necessary in order to avoid the anxiety. With this diminished awareness of stimulation he then would find himself more and more preoccupied with small inconsequential aspects of immediate experience. As these preempted his attention, the effects of

suggestibility would be experienced insofar as he would react freely and uncritically to the residual stimuli.

Related to the above idea is the point that a SD experience could probably be predicted from a marked increase of stimulation. The effect would be to lead the individual to withdraw or become isolated in an effort to insulate himself from the effects of overstimulation. Even when the increase would be of only one type of stimulation, the reaction of the individual might overshoot the mark and his withdrawal and isolation might be generalized to nearly all types or classes of stimulation.

While an individual could initiate a deprivation state either by turning his attention to something of great concern, i.e., other people's reactions to him, or by avoiding most sources of stimulation because of their anxiety provoking qualities as in the case of heterosexual contact, differences in behavioral effects could be expected. The reason is that in the former case the individual would be preoccupied with something that was already of considerable emotional significance to him while in the latter case, the individual would become preoccupied with something that was previously of little or no significance to him.

Any understanding of the dynamics of mental disturbances is incomplete without some explanation of how and why disorders are perpetuated. In connection with this point, three observations concerning behavioral reactions to SD are pertinent. First, SD while producing some pleasurable effects at first soon creates a stimulus hunger (Lilly, 1956). Further, the stimulus hunger is expressed as an increase in desire for stimulation in general as well as in the need for a particular class of stimuli if the deprivation is a selective one. Second, one important by-product of SD is a motivational deficit so that as SD increases it becomes progressively more difficult to carry out or act on wishes and decisions (Hebb, 1958). Third, with prolonged SD hidden methods of self-stimulation (twitching muscles, stroking fingers, and other mannerisms) develop (Lilly, 1956).

These three observations can be tied together by considering again the example of the person who withdraws or avoids a stimulation having to do with heterosexual contact. At first, the initial isolation or withdrawal might bring a feeling of relief or some relaxing of tensions, but as deprivation increased the person would become sensitized to this class of stimulation and would soon experience a sharp increase in the desire for heterosexual contact. But the same deprivation that creates the sensitization and increases the desire for heterosexual contact also weakens the capacity to act on the desire or to carry out the wish for such contact. In other words, as deprivation continued the desire would increase but the drive to satisfy the desire would decrease. Covert methods of self-stimulation, e.g., autoerotic practices or on a more public level some form of manipulative finger play, would then

develop which would represent both a partial satisfaction of the increasing desire and also the limited capacity of the individual to follow through on his desire.

Despite the negative or self defeating aspects of these covert methods of stimulation, they would provide a temporary relief from the conflict created by an increasing desire and the decreasing capacity to act on the desire.

Another way of viewing the self-defeating and self-perpetuating nature of these hidden methods of stimulation is to consider them as distorted expressions of normal desires and wishes. For the real tragedy seems to be that so long as the person experiences normal desires and wishes of strong intensity such maladaptive behavior would continue. In other words, the abnormal desire or wish for withdrawal that initiated the deprivation process would exist no longer, and it would be the return of a normal desire or wish of strong intensity that would perpetuate the maladaptive response in the form of covert methods of stimulation.

The motivational deficit mentioned above may sometimes be partly explained by the fact that the individual experiences certain abnormal feelings and thought during the deprivation process (Lilly, 1956). In the preceding illustration, the need to conceal such abnormality from others might offset the desire to resume heterosexual relationships.

Another point in connection with motivational deficit is that the individual who experienced deprivation as a result of some conflict would have more difficulty in returning to normal stimulation than one whose deprivation was accidental or part of a planned experiment. The reason is that the former would seek such deprivation in an attempt to withdraw or isolate himself. On the other hand, people who experienced some deprivation as part of a creative activity or hypnosis for example would not be trying primarily to avoid certain stimulation and would not be really dissatisfied with the stimuli to which they were customarily exposed. For them deprivation would represent a desire to gain rather than avoid, an escape to rather than an escape from.

In considering the severity of emotional disorder one important variable would be the amount and the duration of the deprivation. Some of the research is consistent in finding that the greater the restriction of sensory input, the more extreme are the behavioral effects (Hebb, 1958; Wheaton, 1959). Also behavioral effects become more pronounced as a given degree of deprivation continues (Solomon, 1957; Wheaton, 1959). In other words, the greater the diminution of external stimulation and the longer the duration of the deprivation, the greater the preoccupation and suggestibility and ensuing symptomatology. Therefore a tentative formulation might be one in which the threshold for neurotic and psychotic reactions would fall at different points on

a deprivation continuum. While the difference between a neurotic and psychotic reaction would reflect a difference in degree of deprivation, this quantitative difference could produce significant qualitative differences as evidenced in the kind of symptomatology and impairment. As has been pointed out elsewhere, even small quantitative differences between two points on a continuum can produce very noticeable and far-reaching consequences. In short, this formulation would allow for both the quantitative and qualitative difference views on mental disorders.

While a certain degree and duration of deprivation combination accounts for a neurotic type reaction and another combination accounts for a psychotic reaction, this formulation does not explain why one neurotic reaction is a conversion syndrome and another is an obsessive-compulsive syndrome, or why one psychotic reaction is a schizophrenic syndrome and another is a stuporous depression. Such an explanation would presumably be sought for in the personality make-up of the individual as well as in the type or types of stimulation of which the individual is deprived.

In connection with the point of having to do with different personalities, markedly different reactions among people subjected to extreme degrees of SD have been reported (Solomon, 1957). There are those who substitute a new world of stimulation through projection of hallucinatory or delusional material. Others are very inactive mentally and do not compensate for the absence of external stimulation by projection or fantasy. Such differential reactions are common in psychotics with some being highly stimulated by their projections or ruminations while others function at a muted low-pitched level of existence. Again, in connection with the point having to do with deprivation of certain types or classes of stimulation, abnormal reactions such as amnesias, multiple personalities, depersonalization may be partly understood in terms of a selective or partial deprivation. That is, the preoccupation with being someone other than oneself might result in the loss of awareness of those stimuli providing superficial identity, and the suggestibility that follows would facilitate the process of selfestrangement as well as believing and feeling oneself to be a different person. Similarly, schizophrenia has been conceptualized as a deprivation of relevancy or meaningfulness of sensory input (Rosenzweig, 1959).

By thinking in terms of different types and forms of deprivation such as emotional, social, intellectual, perhaps even spiritual deprivation, the scope of the SD concept can be widened to subsume a surprisingly large number of psychological phenomena.

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ASPECTS OF TEACHING MACHINE PROGRAMMING: LEARNING AND PERFORMANCE^{1,2}

JOHN A. BARLOW Earlham College

Within the framework of a rather broad interpretation of the learning performance distinction, we shall consider three areas: 1. The reinforcing properties of response confirmation and their effect on learning and performance; 2. Stimulus discrimination and response differentiation; 3. Teaching the student to "think for himself."

In the latent learning studies by Tolman and his followers, claim is made that reinforcement is not essential to learning, as such. However, the "cognitive maps" of the many successors to Blodgett's famous rats were not formed in completely quiescent animals and Tolman has always emphasized the important effect of reinforcement on performance.

Emphasis in the construction of auto-instructional programs (Klaus and Lumsdaine, 1960; Skinner, 1954, 1958) has been on providing a sequence of questions which will lead the student by successive approximations to a mastery of appropriate verbalizations concerning a specific subject matter. There has been little or no direct emphasis on the problem of reinforcing the behavior of working at the program as opposed to reinforcing specific appropriate responses within the program. Response confirmation has been assumed to be sufficient both to reinforce correct student response and to reinforce the more gross behavior of continuing to work at the sequence of items.

Preliminary observations indicate what might have been predicted on a rational or theoretical basis: when the student is presented with a carefully sequenced series of items on each of which correct or appropriate responses are almost inevitable, the effect of confirmation as a reinforcer dissipates considerably after the student has completed several lessons. It would appear that some other type of reinforcer than consistent response confirmation is needed. Errorless sequences may indicate maximum transmission of information, i.e. learning, but themselves do not seem to provide the reinforcement necessary in order to maintain general performance of going from item to item and lesson to lesson.

There is a wealth of evidence (for instance, Ferster and Skinner, 1957) that various forms of intermittent reinforcement are effective

¹ Presented at annual meeting of American Psychological Association, Chicago, 1960.

² Prepared in connection with a research project supported in part by a grant from the U.S. Office of Education, Department of Health, Education, and Welfare (Grant No. 7-12-026.00).

in maintaining a high level of performance. Several individuals have suggested that intermittent reinforcement might be used in connection with auto-instructional programs. If we make a distinction between reinforcing the behavior of giving an appropriate response to a question and reinforcing the more molar behavior of working at the program, intermittent reinforcement appears quite appropriate for maintaining performance at the program providing, "and herein lies the rub", that this does not involve an intermittency of feedback which disrupts the sequence of gradually shaping the desired behavior. (Current work by John Blyth at Hamilton College and by Robert Glaser at the University of Pittsburgh may solve this problem and open entirely new perspectives on the problems of programming.)

A second area in which differences between learning and performance might concern the programmer is inter-relationships between stumulus discrimination and response differentiation. In animal work, we sometimes differentially reinforce a response pattern until the appropriate behavior is established and then bring the behavior under the control of a discriminative stimulus. In view of the complex discriminative repertoire of the human being, it might be very practical to approximately reverse this sequence. If the student can be taught to discriminate between correct and incorrect responses in others and then to discriminate correct and incorrect responses of his own, response differentiation may then proceed with a minimum of further external reinforcement.

Rand Morton has used such a procedure in some of his research on instruction in language skills. He points out: "We, as learners of our native language, begin discriminating among these patterns and responding to them as others around us do. Eventually we learn to use these patterns, to manipulate them, and make them 'habits of speech'." (Morton, 1960, p. 117) ". . . the incipient language learner learns first to 'hear' and discriminate between all 'significant' classes of sounds in the new language before a conscious effort is made to reproduce them." (Morton, 1960, p. 125)

The same type of programming may be fruitful for other skills. In such programming to some extent we reverse the latent learning pattern. Instead of the subject learning (supposedly without reinforcement) and then having this learning made manifest in performance as a result of the introduction of reinforcement, we have a specific discriminative repertoire built up through reinforcement and this repertoire serving as the basis for further modifications in motor behavior.

The last general area we shall consider is that of teaching the student to "think for himself." If we wish to teach the student to think for himself, to emit with increasing frequency idiosyncratic and original responses, how can we possibly provide an auto-instructional device which will be an aid in this case? At first blush, it is inconceivable and

absurd to imagine a machine which is able to reinforce appropriate responses which were unforeseen by the designer of the machine and the designer of the specific auto-instructional program. In fact, it is exceedingly difficult even for the individual teacher in a tutoring situation to successfully shape such behavior.

One situation in which a student is likely to emit idiosyncratic or original behavior (behavior which is based on his own personal reactional biography) occurs when the student is presented with two or more contradictory or conflicting points of view. When the student, for instance, is taking courses in general biology and general psychology at the same time and finds what seem to be contradictory statements presented in the two courses during their sections in genetics, he is provided with a conflict situation. Resolution of this conflict by a closer examination of the evidence and the nature of the seeming contradictions and formulation of a personal opinion is a reinforcing experience. Unfortunately, conventional classroom procedures and conventional test and grading practices tend to extinguish or to actually punish such behavior on the part of the student.

In view of these considerations, we have planned an auto-instructional sequence concerned with interpretation of the Old Testament. (The same procedure would certainly be possible in other areas.) Our plan is: the student will be introduced, for instance, to a specific Humanistic or Naturalistic commentator on the Old Testament and to a specific Fundamentalist commentator. Once he has mastered some minimum essentials of each man's point of view, the student will be presented with a sequence of items concerning various Old Testament passages. The student will be required to anticipate the interpretation of each of the authorities for specific passages and then give his own interpretation. In the case of the shaping of the student's ability to anticipate the two authorities, the programmed sequence will be fairly conventional except for the aspect of establishing two different verbal repertoires and the discrimination between them simultaneously. Our prediction is that the student presented with this situation will show a gradual increase in personal and idiosyncratic responses in this third category of response to each passage. We anticipate that complete absence of punishment of original responses in this situation will act as a sufficient reinforcer to establish and maintain the type of verbal behavior generally called "thinking for oneself."

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THE OPPOSITIONAL NATURE OF DICHOTOMOUS CONSTRUCTS

JEROME RESNICK and A. W. LANDFIELD University of Missouri

Embedded within the assumptive framework of George A. Kelly's *Psychology of Personal Constructs* (1955) are the basic notions that a person's psychological processes are channelized by the way in which he anticipates events, that anticipation is done by construing replications of events, and that a person's construction system is composed entirely of a finite number of dichotomous constructs. "A construct is a two-ended thing, not merely a category of likeness, with no inferred difference in the offing. One cannot refer to the likeness aspect of the construct without simultaneously invoking the difference aspect of the construct" (1955, p. 33). Kelly further states, "Now conventional logic would say that black and white should be treated as separate concepts . . . that a concept is a way in which certain things are naturally alike and that all other things are really different" (1955, p. 106).

According to Kelly, man thinks with contrasts, contrasts which might not be obvious to the external observer but which do constitute meaningful contrasts for a particular individual. Implied in Kelly's conception of contrast is the assumption that contrasts employed by an individual have common characteristics.

In applying this theory, Kelly has developed a technique designed to elicit an individual's personal contrast or construct repertoire. This technique is the Role Construct Test (RCRT). The subject (S) is asked to differentiate triads of acquaintances by stating how two of the three acquaintances are similar and how the third person is different. These alternative descriptions of triads of acquaintances constitute personal constructs. For example, the descriptive statement that two acquaintances are alike in that they are friendly and the third person is not friendly, would constitute a personal construct.

This study is concerned with testing whether the two poles of dichotomous constructs elicited by the RCRT represent the same or different dimensions of experience. Kelly would assume that the two poles of a construct elicited by the RCRT pertain to similar dimensions of experience. If the present investigation were to show that an individual's thinking contrasts may refer to entirely different dimensions of experience, it would tend to bring into question one of Kelly's primary theoretical formulations concerning the nature of thinking processes; and would tend to support the more common notion that a concept is a way in which certain things are naturally alike and that

contrasts may be comprised entirely of things which do not belong on the same dimensions.

This study arose not only on theoretical grounds, but also because of empirical considerations. Certain constant peculiarities have been found to occur in the RCRT's of normal Ss. The oppositional or antithetic nature of the ends of many constructs elicited by the RCRT appear to be logical and obvious and have enough common meaning so as to be highly communicable. However, some constructs so elicited are highly illogical and peculiar with respect to contrast and would seem to invalidate Kelly's assumption regarding the antithetical nature of all personal constructs as elicited by his measure. An example of a peculiar construct would be the following: A person might say that two people are alike in that they are both friendly and that a third person is different in that he has a sense of humor. From the standpoint of the objective observer, it is difficult to see how these particular contrasts can have anything in common or are antithetical. The opposite of friendly, which might be unfriendly, hardly fits with having a sense of humor. Nevertheless, Kelly would assume that friendly and sense of humor are really opposite characteristics for the individual.

Not only have peculiarities in contrasting been noted on the RCRT, but also peculiarities in contrasting people are quite noticeable in the verbalizations of persons undergoing psychotherapy. For example, one client contrasted a happy friend with one who might commit suicide. If happy and suicidal were understood to have some commonality of meaning, and further, if one assumes with Kelly that an individual's behavior can be plotted most meaningfully in terms of an individual's own set of contrasts, one would certainly hypothesize that suicide might be the most likely alternative to happiness for the client himself.

To further emphasize the importance of understanding the nature of contrasting descriptions, the following clinical case is described briefly, in conjunction with personal constructs used by this client: Client X is male, married, thirty years of age and a candidate for an advanced academic degree. Outwardly, X is passive, dependent and anxious to be accepted by others. He is having academic difficulties and worries about his adequacy as a student. His primary symptoms are gastro-intestinal complaints. Personal constructs, elicited by the RCRT, are as follows: loving vs. dumb; mean vs. studious; tolerant vs. unjust; foolish vs. reasonable; well-informed vs. repulsive; immature vs. democratic; fascinating vs. uncertain; considerate vs. unsympathetic; decisive vs. uncertain; cruel vs. wise; thoughtless vs. liberal; happy-go-lucky vs. dominating; intelligent vs. aimless; clever vs. anti-social; careful vs. popular.

If one assumes that the foregoing contrasts actually have a common core of meaning, and further, if one assumes that these contrasts repre-

sent axes along which one may understand the client's own behavior, many interesting clinical hypotheses are suggested. For example, if X begins to doubt his intellectual abilities, he might become unloving, mean, repulsive, unfascinating, indecisive, cruel, aimless and anti-social. This type of hypothesis is appropriate if we can show experimentally that peculiar contrasts do in fact have some common core of meaning. As far as this clinical case is concerned, we do have some observational evidence that the above hypothesis is a good one. For example, X did become aimless, vague and indecisive in his work, openly hostile toward his family and showed impulsive, aberrant sexual behavior in the form of voyeurism. This behavior was concurrent with his report in therapy of severe doubt concerning academic competence.

RATIONALE

What might be one criterion upon which the degree of opposition of the poles of dichotomous constructs can be measured? For two things to be considered opposite from each other, they must be opposite with regard to the same underlying basic characteristic. To say that "good" is the opposite of "bad" implies that goodness and badness both stem from the identical underlying evaluative characteristic.

If we were now to accumulate an array of prospective dimensions or properties which a symbol could be thought of as having, and ask S to tell us which dimensions or properties apply to a particular symbol he used, this should give us some indication of his thinking as to what the symbol means to him. We will use as this array of prospective properties or dimensions a series of forty adjective pairs called the Adjective Pair List, with each adjective in a pair being presented as the defined opposite of the other. For example: Fast—Slow, or Pleasant—Unpleasant. Each adjective pair, therefore, may be looked upon as a conceptual dimension. Osgood, Suci and Tannenbaum (1957) have contributed importantly to our thinking here.

Let us then select two constructs elicited by the RCRT which S has used, and represent these constructs to him as four separate symbols. One of the constructs to be represented will be as logical and highly meaningful as can be found (e.g., mature vs. immature). And one will be peculiar in logical opposition (e.g., mature vs. religious). Let us also ask S to select ten adjective dimensions from the Adjective Pair List which he feels can best describe or characterize each of the four individual symbols.

If S regards his two symbols in a construct as being opposite, then he should choose for one symbol (e.g., mature) the same conceptual dimensions that he chose for the other symbol (e.g., immature). It makes no sense, if S regards his symbols in a construct as being opposite, if he uses one set of ten dimensions with which to describe one

symbol and an entirely different set of dimensions with which to describe the other symbol.

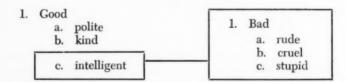
On the basis of the reasoning so far developed, we hypothesize that the overlap between conceptual dimensions or aspects chosen to describe the two symbols in a construct should be significantly better than chance expectation, if Kelly's theory is to be supported.

Should this hold true in the case of those constructs whose opposition or contrariety appears to be highly illogical, such as in the construct: *intelligent* vs. *bad?* Should there not be far more dimensional overlap for a construct such as *intelligent* vs. *stupid*, a construct whose logical opposition is far more clearly implied? A clue as to the thinking underlying the formation of such a construct is given by Kelly:

One construct may subsume another as one of its elements . . . For example, the construct "good vs. bad" may subsume, respectively, among other things, the two ends of the intelligent—stupid dimension. In this sense, 'good' would include all intelligent things plus some things which fall outside the range of convenience of the *intelligent* vs. *stupid* construct. 'Bad' would include all the 'stupid' things plus some others which are neither 'intelligent' nor 'stupid' (1955, p. 57).

Diagrammatically, what might occur in the case of the peculiar construct *intelligent* vs. *bad*, based upon the above theorizing, is shown in Fig. 1.

FIG. 1. BIPOLAR CONSTRUCT



The construct intelligent vs. bad is formed by one of the subordinate elements of good plus the entire superordinating system of bad. If this is the case, then we should expect more than just chance overlap of common aspects between intelligent vs. bad, but certainly not as great a common bond of aspects as might be obtained between a more logical construct such as good vs. bad or intelligent vs. stupid.

Now, it goes without saying, that if S regards the two symbols in a construct as being opposite and contrasting, then he should choose for one symbol—not only the same dimensions that he uses for the other symbol (which is the main theme of this study)—but, in addition, within these conceptual dimensions which overlap the two construct ends, he should use one adjective extreme for one construct symbol, and the other adjective extreme from that particular pair, for the other construct symbol. If, for example, S uses eight identical dimensions with which to rate both construct ends, but uses the same adjective for one construct symbol that he uses for the other, for all eight dimensions, then, though these two construct ends are closely related, their relationship is one of identity and similarity, rather than contrast and opposition.

HYPOTHESES

- 1. The overlap or commonality of conceptual dimensions chosen to describe the two ends of a *logical* personal construct should be significantly (.05 level) greater than chance expectation.
- 2. The overlap or commonality of conceptual dimensions chosen to describe the two ends of a *peculiar* personal construct should also be greater than chance expectation.
- 3. There should be significantly more overlap of dimensions for the *logical* constructs than for the peculiar constructs.
- 4. Where overlap of conceptual dimensions chosen to describe the two ends of a personal construct is found, opposite adjective extremes should be selected in describing these construct ends.

METHOD

Subjects. The experiment was conducted over two sessions, and all conclusions drawn from the results of this study are based upon those Ss who completed both sessions. Fifty-two Ss were used in the first session, and of these, 41 Ss completed the second. These 41 Ss were undergraduate students at the University of Missouri; fifteen of whom were drawn from the Special Section of a course in General Experimental Psychology, and 26 drawn from a course in Social Psychology. Of the 41 Ss, 15 were males and 26 females. The experiment was conducted in the regular sessions of the classes in which the students were enrolled.

Measuring Devices. A slightly modified (shortened) form of Kelly's RCRT, Group Form consisting of a Role Specification Sheet and a Response Sheet was used, in which triads of 15 acquaintances were described. This particular RCRT method is well described in an article by Bieri (1955).

The second measuring device used was an Adjective Pair List (See Table 1) consisting of 40 pairs of descriptive adjectives listed in two columns down a single sheet of paper. The adjective pairs used in the

study met the following requirements: They (A) were words with high cultural meaning and usage (Osgood, 1957), (B) were checked in a dictionary to insure accuracy of oppositeness, and (C) were inspected by the E in an attempt to insure a minimum of overlap in meaning from one pair to another. Two forms of this Adjective List were used. The forms differed solely in the ordering of listing and not in content. The ordering of listing of the adjective pairs of Form One was determined through a randomized procedure. Having set the order of Form One, the order of Form Two was then determined by a double simultaneous reversal of the ordering of pairs of Form One. This resulted in the first column of Form One becoming the second column of Form Two, and the adjectives on the bottom half of Form One becoming the adjectives at the top half of Form Two. One-half the Adjective Pair Lists used were Form One type; one half, Form Two.

TABLE 1. ADJECTIVE PAIR LIST

(Concept to be rated:			
1. Calm	:Agitated	21.	Valuable	:Worthless
2. Rich	:Poor	22.	Colorful	:Colorless
3. Ornate	:Plain	23.	Superior	:Inferior
4. Humorous	:Serious	24.	Dynamic	:Static
5. Hard	:Soft	25.	Bright	:Dark
6. Large	:Small	26.	Fertile	:-Sterile
7. Simple	:Complex	27.	High Class	:Low Class
8. Beautiful	:Ugly	28.	Pleasant	:Ugly
Abrasive	:Oily	29.	Wet	:Dry
10. Polite	:Rude	30.	Fast	:Slow
11. Feminine	:Masculine	31.	Positive	:Negative
12. Active	:Passive	32.	Sincere	:Insincere
Graceful	:Awkward	33.	Healthy	:Sick
14. Heavy	:Light	34.	Constrained	:Free
15. Hateful	:Lovable	35.	Clean	:Dirty
16. True	:False	36.	Fresh	:Stale
	:Trivial		Deep	:Shallow
18. Ordered	:Chaotic		Precise	:Vague
-	:Disagreeable		Humble	:Arrogant
20. Sacred	:Profane	40.	Strong	:Weak

Procedure. A time interval of exactly one week separated the two sessions of the experiment. Very generally summarizing and overviewing the procedure: During the first session, each S took the RCRT. This yielded a series of 15 bipolar constructs for each S.

From each S's protocol of 15 constructs, two constructs were selected by a panel of three judges, one peculiar construct and one nonpeculiar construct. During the second session, each S rated or described these two constructs against a series of descriptive adjective pairs. For example, an S might have rated the four construct ends (symbols): mature, immature, intelligent, and bad—rating each symbol separately against an Adjective Pair List. In other words, each S used four adjective pair lists, one for each contrast end of a personal construct.

During the first session, each S was given a copy of the RCRT, along with a typed set of instructions. Ss were told not to sign their names. A system of code numbers was used to insure that Ss received back their own constructs to rate during the second session.

The two constructs from each protocol were selected by the three independent judges. First, each judge was to indicate those constructs which appeared to be most clear cut, <code>logical</code> and obvious in the oppositeness of the ends of the construct. Second, each judge was also to indicate those constructs which appeared to be most illogical and <code>peculiar</code> with regard to the oppositeness of the ends of the construct. Two examples of <code>logical</code> constructs selected were: mature vs. immature and optimists vs. pessimists. Examples of the <code>peculiar</code> were: sense-of-humor vs. out-going; artistic vs. happy-go-lucky. Borderline constructs (between obvious and peculiar) were placed in a third category.

Constructs were chosen which met the following minimal criteria: Two of three judges agreed and the disagreeing judge rated the construct as ambiguous as to logicalness or peculiarity. First preference, however, was given to those constructs which had unanimous agreement.

In the case of 40 Ss, the logical construct selected was a result of unanimous agreement. The one remaining logical construct was selected as a result of two-thirds agreement. For the peculiar constructs, 27 of these were unanimously selected, and 14 as a result of two-thirds agreement.

At the beginning of the second session, four rating sheets were given to each S. At the top of each sheet was written a concept end from his own RCRT in the first session. If one construct end appeared on Form One of the Adjective Pair List, the other construct end always appeared on Form Two. Instructions were then read aloud to the group, with illustrations of the instructions demonstrated on the classroom blackboard, where appropriate. Ss were asked to think of each adjective pair as a scale and to rate the word or phrase (S's construct end) written at the top of the page. Ss were instructed to choose no more than, nor less than ten adjectives (within ten adjective pairs) with which to describe the concept end.

RESULTS

For each S, the number of overlaps between dimensions chosen for both construct ends was tabulated. Thus, for example, if an S checked the *true* vs. *false* dimension as being one dimension which could describe one construct end, and similarly checked the *same* dimension for the other construct end, this constituted one overlap for that construct. As Ss were to choose ten dimensions for each construct end, the maximum possible overlap was ten. Having 40 dimensions available, and instructed to choose ten of the 40, by chance alone, we should expect an average overlap among Ss of 2.5.

Two distributions of overlaps were tabulated: one for the *peculiar* constructs and one for the *logical* constructs. The means and standard deviations of these distributions are presented in Table 2. These results were subjected to a one-tailed t-test on the hypothesis that the number of overlaps will be greater than chance. For the distribution of overlaps for the *logical* constructs, the chance expectancy of getting the obtained M of 7.24 was less than .0001, supporting *hypothesis* 1: the overlap of conceptual dimensions chosen to describe each end of a *logical* personal construct should be significantly better than chance expectation. Similarly, the mean number of overlaps obtained for the *peculiar* constructs (M=4.76) indicated significance beyond the .0001 level of confidence, supporting *hypothesis* 2: the overlap of conceptual dimensions chosen to describe each end of a *peculiar* personal construct should be significantly better than chance expectation.

TABLE 2

DIMENSIONAL OVERLAP BETWEEN CONCEPTUAL POLES
OF LOGICAL AND OF PECULIAR CONSTRUCTS

Construct category	Mean	SD	t	P
Logical	7.244	1.319	23.03	.0001
Peculiar	4.756	1.609	9.279	.0001
Difference-Logical and peculiar	2.487	1.658	9.606	.0001

For each S, the difference between the number of overlaps for the peculiar and logical constructs was computed. Thus, for example, if an S had three dimensions overlapping for his peculiar construct, and seven overlaps for the logical, the difference in overlaps was four. Presented in Table 2 is the mean difference in overlaps for the 41 Ss along with the S.D. of this distribution. This mean difference, significant at the .0001 level (t=9.606), supports hypothesis 3: the overlap of conceptual dimensions on logical constructs should be greater than on peculiar constructs. Finally, a check was made to determine whether Ss used the same adjectives or the opposite adjectives in the overlapping dimensions to rate the two construct ends. In the case of the peculiar constructs, 31 Ss out of the 41, used opposite adjectives when they used the same dimension for both construct ends, 100 per cent of the time. That is: 31 Ss had a perfect score of choosing one adjective for one construct end, and then choosing the opposite adjective in that dimension for

SUMMARY AND CONCLUSIONS

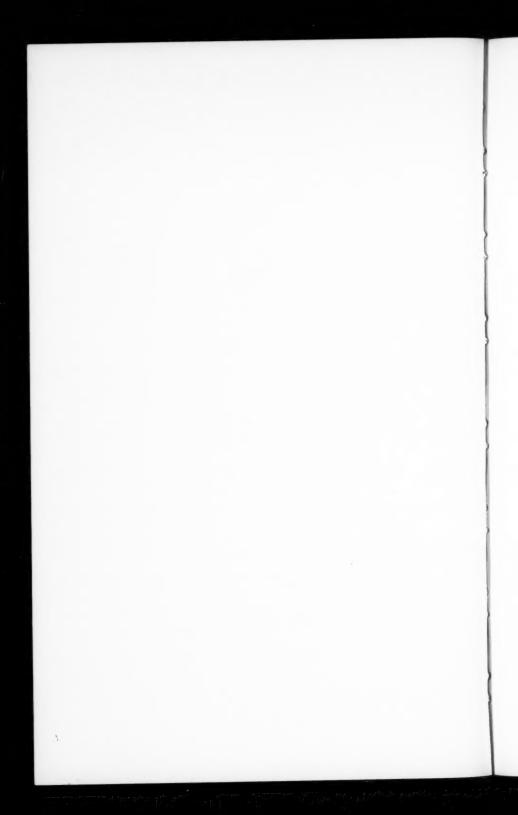
The RCRT was administered to a group of Ss, and from each S's protocol, two constructs were selected by a panel of three judges. One of these constructs was clear cut, *logical* and obvious in the dichotomy or antithetic nature of its ends; one was highly illogical and *peculiar* with regard to apparent contrast. A rating task was formulated whereby Ss were to indicate which ten adjective pairs from a list of 40 pairs best described each of the four construct ends.

Four major hypotheses were formulated: (1) The overlap or commonality of conceptual dimensions chosen to describe each end of a logical personal construct should be significantly better than chance expectation; (2) this should also hold true for the peculiar constructs; (3) there should be significantly more overlap of dimensions for the logical constructs than for the peculiar constructs; and (4) where overlap occurs, personal construct ends should be rated oppositely on these common dimensions. All hypotheses were confirmed.

This study supports Kelly's contention that contrasting ends of personal constructs represent similar dimensions of meaning. In fact, even those constructs in which contrasts do not seem antithetical (to the external observer), have some common meaning. More generally, our findings support Kelly's basic position that a concept should be defined in terms of differences as well as similarity, with the assumption that differences have a commonality of meaning.

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THE RELATIONSHIP BETWEEN STIMULUS INTENSITY AND THE ELECTRICAL RESPONSES OF THE COCHLEA AND AUDITORY NERVE¹

W. L. GULICK, D. J. HERRMANN, and P. E. MACKEY

University of Delaware

Since the discovery of the cochlear response by Wever and Bray (1930a, 1930b), there has been lengthy speculation among auditory theorists as to the role of this electrical response in hearing. Almost from its discovery, Wever (1949) has maintained that the cochlear response excites the nerve fibers of the auditory nerve. Stevens and Davis (1938), on the other hand, have suggested that the electrical response of the cochlear is irrelevant to hearing. According to these experimenters the cochlear response is an epiphenomenon. Their view included ascription of the response to a microphonic effect which occurred independently of physiological processes.

The supposition that the electrical response of the cochlea is a microphonic potential is unlikely. The electrodynamic properties of the hair cells of the organ of Corti have been likened to a piezoelectric crystal which converts sound waves into electric currents (Stevens and Davis, 1938). Upon examination this seems a poor analogy because the energy transformation in a crystal depends upon a peculiar structure consisting of molecules in a rigidly systematic orientation. The hair cell does not appear to have such a structure: the response is not a microphonic potential.

Other lines of evidence also deny the microphonic hypothesis of Stevens and Davis. Békésy (1952) has calculated that the mechanical energy of sound cannot account for all of the electrical energy of the cochlear potential. This, of course, indicates that the generation of the response is based upon metabolic processes. In 1952 Békésy discovered the endolymphatic potential, and it has been suggested that this potential is the source of energy for the cochlear response (Davis, 1957). It seems generally agreed upon now that the cochlear response is a true physiological response rather than a simple microphonic potential, and its dependence upon oxygen metabolism and normal body temperature lends further support to this view (Chambers and Lucchina, 1956; Gulick, 1958; Gulick and Cutt, 1960; Kahana, Rosenblith, and Galambos, 1950; Wing, Harris, Stover, and Brouillette, 1952).

¹ From the Physiology Laboratory, Department of Psychology. This research was supported, in part, by a grant from the University of Delaware Research Foundation.

The question of the relevance of this response to hearing remains. It has been known for a long time that there are four basic energy forms capable of exciting neural tissue, and these forms are thermal, mechanical, chemical, and electrical. It is clear that the ear, functioning as a mechanical receptor, must transduce acoustic energy into one of these forms in order to initiate neural impulses in the auditory nerve. Historically, both the chemical and the electrical forms of trigger action have been given serious attention as possible mediators of neural excitation. The initial interpretation of the cochlear response given by Stevens and Davis (1938) led them to deny the electrical response of the cochlea as a physiological trigger important to hearing. In its stead they offered an alternative; namely, that impulses in the auditory nerve were triggered chemically by a stimulant which was liberated during acoustic stimulation of the ear. It was the discovery of a relatively long latency of the first signs of nerve activity following the onset of stimulation of the ear that tended to favor the chemical mediation hypothesis (Derbyshire and Davis, 1935). As measured, the latency strongly suggested the presence of a chemical reaction which occurred in the interval between cochlear stimulation and subsequent neural activity. However, it appears now that the latency is shorter than it was previously believed to be (Davis, Tasaki, and Goldstein, 1952). The early measures were apparently erroneous because insufficient account was taken of conduction time from the dendritic processes to the locus of the recording electrode. At present the data show that the latency of the nerve action potential is very short, and this fact argues for rather than against the direct electrical mediation hypothesis. This is the view taken by Davis in his recent work (1957, 1958).

Beside the matter of latency there are several reasons why the electrical hypothesis of nerve stimulation is to be favored over the chemical mediation hypothesis. First, the presence of the electrical response from the cochlea is undeniable, and its magnitude is surprisingly large. Second, the response occurs at the immediate site of the dendritic processes of the auditory nerve fibers within the cochlea. Unless it can be shown that these neural processes are insulated, it seems inevitable that they would be stimulated by the cochlear response. It is possible, as Wever (1949) and Davis (1957) have pointed out, that some chemical factor also operates in the release of energy held by the nerve fibers. But if a chemical stimulant is involved as part of the triggering action of the cochlea, it would tend to be relatively unimportant since the stimulant would encounter neural tissue that was already in the process of responding.

If the cochlear response is an electrical trigger for the auditory nerve, then changes in the magnitude of this response should produce systematic changes in auditory nerve stimulation. The purpose of the present experiment was to make inquiry into the relationship between the magnitude of the cochlear response and the magnitude of the compound nerve potential representing the response of first-order neurons in the cochlear branch of the eighth cranial nerve. A correlation between response magnitudes does not necessarily imply causal relations between them. Nevertheless, the establishment of a correlation would be completely consistent with the electrical hypothesis and, in a general way, supportive of it.

METHOD

Four adult guinea pigs were employed as experimental animals. Body weights ranged from 350 to 550 grams. Their daily diet consisted of about 35 grams of rabbit chow (Purina) and a plentiful supply of fresh greens. Water was always available and the average daily intake was 50 cc.

Anesthetization. Anesthesia was produced with ethyl carbamate (Urethane, U.S.P., Merck) in 20 per cent aqueous solution (pH=6.0). The anesthetic was injected intraperitoneally in a dosage of 11 cc per kilogram of body weight. The dosage was sufficient to produce a surgical level of anesthesia within one hour and to maintain the preparation in good physiological condition for the duration of the experiment (up to four hours after injection).

In one animal the anesthetic caused respiration to become depressed, and in order to prevent respiratory failure 0.25 cc nikethamide (Coramine, U.S.P., Ciba) in 25 per cent aqueous solution was injected intraperitoneally.

Surgical Procedure. The tympanic bulla was approached laterally through a 1 cm incision in the skin immediately posterior to the base of the pinna. Dissection was begun at a point 1 cm dorsal to the posterior tip of the mandible. This entry gave access to a part of the mastoid portion of the temporal bone anterior to the lambdoidal ridge. After removal of the periosteum, a hole (diameter 1 mm) was drilled with a small burr until the middle ear cavity was reached. The bone in this region is usually less than 0.5 mm thick. The proper place of drilling was midway between the lambdoidal ridge and the foramen of the external auditory meatus. The opening was then carefully enlarged with forceps. Thereafter the endosteum was punctured to expose the round window membrane of the cochlea.

Method of Stimulation and Recording. The stimulus used in this experiment consisted of an acoustic click produced by a stimulus generator (Grass, Model PS-2) which provided a 10 microsecond pulse upon external triggering. The signal from the stimulator fed into an attenuator (Hewlett-Packard, Model 350-B) with a range of 110 db, variable in steps of 1 db. The output from the attenuator led directly to a 12 cm cone loudspeaker housed in a soundproof chamber.

Aerial sound was conducted into the external meatus through a

tube leading from the speaker chamber. A cannula at one end of the tube was sealed within the external meatus so that the tip of the cannula was approximately 4 mm from the tympanic membrane.

The active electrode was made from a piece of platinum foil (2.5 micra thick) cut in the form of an isosceles triangle with the base equal to 1 mm and the altitude 2 mm. The base of the foil was soldered to the end of a nylon insulated copper wire (36 gauge). The inactive electrode, made from a steel hypodermic needle, was inserted in exposed tissue surrounding the incision.

The tip of the recording electrode was placed upon the niche of the round window by means of a micromanipulator. Both the cochlear response and the nerve response were picked up with the electrode on the round window membrane. Recordings taken from this site have two prominent components. The earlier and more prominent wave is the cochlear response. This may be distinguished from the nerve response in that changes in polarity of the stimulus produce changes in the direction of the deflection of the cochlear response but not of the nerve response (Brazier, 1953). The responses were amplified and recorded graphically with an audio frequency spectrometer equipped with a recording attachment (Brüel and Kjaer, Model 2109). During stimulation and recording procedures each animal was completely isolated in an electrically shielded room.

Procedure. After anesthetization and surgery the tympanic bulla was drilled to expose the round window. Then the sound tube cannula was inserted into the external meatus. A tight seal was effected by wrapping the pinna around the cannula and taping it with plastic tape.

For the duration of the experiment each preparation was maintained at normal body temperature $(37^{\circ}C.\pm1)$. Body temperature was measured with a laboratory thermometer placed under the side of the animal as he lay on the operating table within the shielded room. This method of measuring body temperature proved to be as accurate as an intraperitoneal placement, and the ease which it afforded made it considerably more satisfactory.

After the placement of the thermometer and the electrodes, the acoustic stimulus was introduced into the ear. Typically, the experimental procedure began with a stimulus with its intensity attenuated sufficiently so as to produce no measurable cochlear or nerve response. The intensity of the stimulus was then increased in 5 db steps until the limit of the apparatus was reached. At each intensity level the acoustic click was presented eight times, at a rate of about one per second. The entire procedure was repeated four times for each animal, and 15 minutes elapsed between successive replications.

Data Analysis. In Figure 1 there may be seen a representative tracing of the cochlear response (CR) and the eighth nerve response

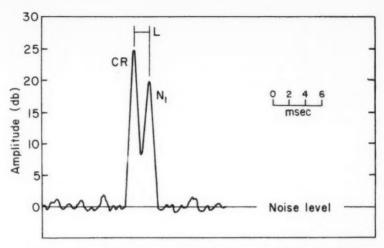


Figure 1. A representative tracing of the cochlear response (CR) and the eighth nerve response (N_1) recorded from the round window membrane of a guinea pig. The stimulus was an acoustic click delivered to the ear by the closed-tube method. Response amplitudes are given in debicels relative to noise level, and the latency (L) of the nerve response is given in msec.

 (N_1) to an acoustic click. Peak amplitudes were measured graphically and were expressed in decibels relative to noise level (approximately 2 microvolts). The latency of the compound action potential was measured in milliseconds from CR peak to N_1 peak.

RESULTS

The data obtained on each of four guinea pigs indicate that there is a systematic relationship between the magnitudes of the cochlear and N_1 responses. The nature of this relationship is apparent from the representative data presented in Figures 2 and 3. Proportional increases in the cochlear response are accompanied by proportional increases in the N_1 response over most of the range investigated. Both responses are given in decibels relative to noise level. The open circles represent measures taken on the final replication, and they are presented so that a comparison of final measures with initial measures (solid circles) can be made. The fact that very little difference exists between them is taken to mean that the preparations remained in good physiological condition throughout the experiment. Each point plotted in Figures 2 and 3 is the mean amplitude of the eight measures obtained under each stimulus intensity condition. The variation in response magnitude within a single intensity condition was small (± 1 db).

The latency of the compound action potential did not vary as a function of stimulus intensity; rather, it appeared to remain relatively constant at about 1.9 msec.

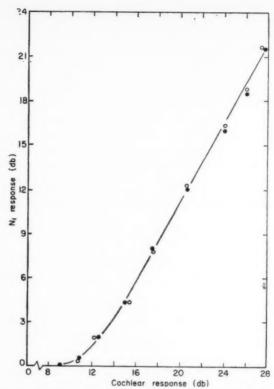


FIGURE 2. The relationship between the magnitudes of the cochlear and the eighth nerve responses of a guinea pig. Data obtained on one animal. Both responses are given in decibels relative to noise level. Each point represents the mean response amplitude of eight measures taken in a single intensity condition. The solid circles and open circles represent initial and final measures obtained under identical conditions.

DISCUSSION

Before considering the relationship found to exist between cochlear and N_1 responses, let us first attend to the action of the hair cells of the organ of Corti. The cochlear response represents a composite picture of the responses of all of the active hair cells. It is known that the composite potential produced by a pure tone stimulus varies as a linear function of sound pressure up to the point of overloading. Overloading probably involves mechanical distortion of the supportive structures so that the hair cells are no longer deformed in proportion to stimulus intensity. When the ear is driven too hard and overloading occurs, the cochlear response departs from linearity and becomes decelerated.

Beginning with the fact that the cochlear response bears a linear relationship to sound pressure, we may consider the nature of the response of individual hair cells. It may be that the output of individual hair cells is progressively decelerated with increases in stimulus intensity. But if this were so, then the linear composite function obtained experimentally could occur only if active hair cells were added in such a way as to compensate exactly for the decreased voltage occurring as a result of deceleration. That this combination of events would obtain oven an extensive range (about 60 db) is so unlikely as to invite no further consideration.

A more tenable hypothesis to account for the linear form of the intensity function is that the voltage output of the individual hair cell bears a linear relation to sound pressure and the number of hair cells

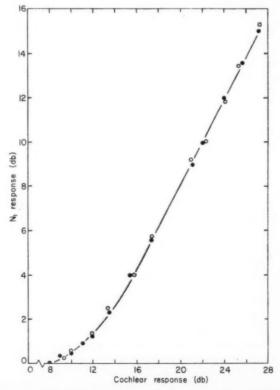


FIGURE 3. The relationship between the magnitudes of the cochlear and the eighth nerve responses of a guinea pig. Data obtained on one animal. Both responses are given in decibels relative to noise level. Each point represents the mean response amplitude of eight measures taken in a single intensity condition. The solid circles and open circles represent initial and final measures obtained under identical conditions.

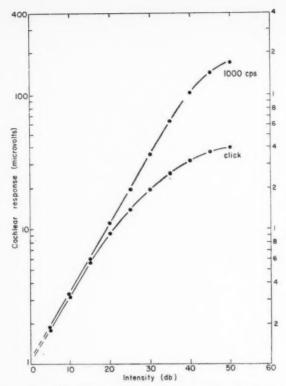


FIGURE 4. Intensity functions showing the cochlear response in microvolts to a 1000 cycle pure tone and an acoustic click. Intensity is expressed in decibels relative to the output voltage at the oscillator (1000 cycle tone) and stimulus generator (click) used to produce the smallest measurable response. These voltages were 4 and 8 millivolts, respectively.

in action does not increase with increases in stimulus intensity. If each hair cell responds linearly, then the assumption of the addition of active hair cells with intensity increments could only result in a positively accelerated composite function. Since the data contradict this assumption we must conclude that all the hair cells are active, no matter what the stimulus intensity. This is not to conclude, however, that all the hair cells are responding equally to a given stimulus.

Let us turn now to the compound potential of the eighth nerve. The representation of stimulus intensity in the peripheral nervous sysem is assumed to be the number of neural impulses per unit of time. When a threshold stimulus is applied to a single fiber, its rate of firing is determined by the duration of its absolute and relative refractory periods. However, at intensities slightly above the rheobase, the fiber no longer requires complete recovery but can respond during the rela-

tive refractory period. In fact, as intensity is raised further, the fiber will fire earlier and earlier in the refractory period. Except for the limit imposed by the absolute refractory period, the rate of firing in a single fiber increases with stimulus intensity (Galambos and Davis, 1943).

Under conditions wherein a population of neurons is under stimulation, as in the eighth cranial nerve, it is assumed that fiber thresholds are distributed normally. Nevertheless, the rate at which each individual fiber fires once its threshold has been reached increases with stimulus intensity, although not necessarily at the same rate of increase. Accordingly, the total number of impulses per unit time, taking all active fibers into account, also increases with stimulus intensity.

In the very early stages of continuous stimulation, the magnitude of the spike potential decreases upon repeated firings and then stabilizes until neural fatigue begins. This equilibration phenomenon was not evidenced in the present experiment because the acoustic click was too brief a stimulus. Individual fibers probably did not fire more than once for each click, and since the rate at which the stimulus was presented was slow (once per second) it is likely that even the slowest fiber to recover had more than sufficient time for complete re-polarization. In terms of the foregoing discussion it is suggested that the primary determinant of the N₁ magnitude in the present experiment was the number of active neurons. Since the stimulus was very brief the complication of equilibration is not relevant. Moreover, the nature of the stimulus also makes a consideration of the rate of firing of individual fibers fall outside the scope of immediate concern.

Data obtained in this study indicate that neurons are added to the total population of active fibers in a fairly regular manner as stimulus intensity is increased. When cochlear and N_1 amplitudes are expressed in decibels relative to noise level, as for example in Figure 2, the relationship between them appears to be linear over most of the range. It should be pointed out, however, that the slope of the function describing this relationship typically exceeded 1.0 in the present experiment, thereby demonstrating that the function is actually positively accelerated when the responses are plotted directly in voltages. If it is so that the thresholds of the eighth nerve fibers are normally distributed, then one would predict that at low intensities the rise in N_1 magnitude due to the addition of neurons would be positively accelerated as a function of either cochlear response magnitude or stimulus intensity. The data support this prediction.

When a click is used to stimulate the ear, the intensity function obtained is dissimilar to the one noted under pure tone stimulation. The major differences lie in a reduction of the intensity range through which the cochlear response remains linear and in a more gradual deceleration.

Figure 4 presents two intensity functions. One is the cochlear response produced by stimulation with a 1000 cycle tone, whereas the other is the response given to an acoustic click. Wever and Lawrence (1954) have shown that the simultaneous delivery to the ear of two or more tones produces distortion within the cochlea which is reflected in reduced cochlear output. The reduction is believed to be the result of transformation of some of the energy into over-tones (aural harmonics) and interference. Probably the same phenomena would occur upon stimulation by an even more complex acoustic signal such as a click.

The early departure from linearity noted in the function obtained to the click probably reflects a diversion of energy rather than a loss, as in overloading. It is possible, however, that overloading is partly responsible. A spectral analysis of the click used in the present experiment indicated it to have some frequency components in excess of 4000 cycles per second. Since the linearity range for high frequencies is known to be short, it is possible that the early departure from linearity in the relatively small population of hair cells maximally activated by the high frequencies is responsible for the gradual deceleration of the composite function. That the composite function departs gradually indicates that the majority of hair cells—those representing the lower frequencies—continued to operate linearly.

SUMMARY

The cochlear response (CR) and the compound potential of the first-order neurons of the eighth cranial nerve (N_1) were recorded from the round window membrane of each of four adult guinea pigs during auditory stimulation by an acoustic click delivered to the ear by the closed-tube method. The results indicated that proportional increments in the magnitude of the electrical response of the cochlea are accompanied by proportional increments in the magnitude of the nerve response. The latency of the N_1 response was not influenced systematically by stimulus intensity. The relationship noted between these two responses is consistent with the electrical hypothesis of auditory nerve stimulation.

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IS THE SYSTEM APPROACH OF ENGINEERING PSYCHOLOGY APPLICABLE TO SOCIAL ORGANIZATIONS?¹

THOM VERHAVE

^aIndianapolis, Indiana

"The responsibility of philosophical inquiry is to help release and mature the often pent-up, half-articulate yearnings of men to enrich the quality of their existence, and to aid in discovering the personal and social instruments through which these yearnings may achieve some increment of substantial realization."—Max Otto

The main ideas of this paper stem from four sources: 1) cybernetics, or the theory of servo-mechanisms, as developed since World War II by Norbert Wiener, Claude Shannon, Warren Weaver, Ashby and many others; 2) the basic ideas of the system approach developed by Engineering Psychology (Taylor, 1960); 3) the writings concerned with the problems of industrial management; and 4) the facts and principles of operant behavior.

Since an understanding of the essential features of what follows depends on an understanding of a few basic concepts, a brief outline of the key terms will be given.

For purposes of the present discussion, the term "social system" refers to any group of people in which we can distinguish two subgroups. Each of the subgroups has its own appropriate repertoire consisting of a set of specific behaviors. A classroom can be conceived of as such a social system. The pupils are one of the subgroups and the teacher is the other subgroup with a membership of n=1.

Although the basic ideas of this paper can be extended to any social system, the present discussion will limit itself to systems existing in industrial organizations.

Most of the literature concerning employee-management relationships has paid only lip service to the reciprocal influence between subgroups within any social system. They interact, which is another way of saying that each group controls the other. Much of the literature on industrial relations, however, ignores this fact. The discussions pro-

¹ This is a modified and expanded version of a paper presented at a symposium on Applications of Behavior Technology held under the auspices of the American Association of Advancement of Science at New York City on December 30, 1960.

^{* 4111} East Pleasant Run Parkway, South Drive

ceed as if the control is in one direction only: management sets the policy for the employees. Social power, broadly defined as the ability to control behavior, for example, by means of the ability to dispense rewards and punishments (Bass, 1960, p. 222), is thought of as unilaterally and vertically organized (Katz, 1960). The issues discussed in the management literature mainly involve problems and disagreements concerning the best or most efficient techniques of control and the possibilities for developing new "tools of power": new wage-incentive plans, how to bargain with unions, the rights of management, etc.

Such neglect of the basic facts of a continuous mutual interaction has recently become more and more recognized from various corners (Argyris, 1957; Bauer, 1960; Katz, 1960; Skinner, 1953). It is exactly the existence of a continuous and mutually interacting relationship between employees (or unions) and managers which makes the basic concepts of cybernetics relevant to even a rudimentary understanding of the behavior of people in an industrial organization or any other social system.² Robert L. Katz (1960) has given a general description of the consequences within an organization that can be attributed to the lack of appreciation of "bilateral control" on the part of management.

A key concept in the discussion that follows is the notion of feedback. The notion of a feedback control system is illustrated by the governor of a lawn mower engine. A flywheel mounted on the crankshaft of the engine functions as a blower. The force of the air stream depends on, or is controlled by, the speed of rotation of the motor. The air stream blows against a lever connected by means of a wire and pulley system to the throttle of the engine which, of course, controls the speed of the engine directly.

In a feedback control system, therefore, some effect produced within the system (the air blast) by another part of the system (the engine) is fed back into a component of the system (the throttle), which is made to control in turn that part of the system (the engine) which generated the result (air blast) in the first place. The appropriateness of the feedback control system terminology to the social systems mentioned can be put as follows: The activities, decisions and directives of management control, at least partially, the behavior of the employees. The particular way in which the employees react in turn affects the further actions of management.

The conditions existing within the employee-management system (E-M system), however, are more complex than the one described in our simple feedback example of the lawn mower. The E-M system is a multiple feedback system. But the basic fact remains: the behavior

^{2.} For a general introductory discussion of the concepts of cybernetics and their relevance to behavior see W. Sluckin: Minds and Machines, 1960.

of the employees may directly influence decisions on the part of management, but the way management reacts may feed back to the employees and again change their course of action.

Analogies between mechanical and electronic feedback systems and social situations are nothing new. The literature in this area is large and widely scattered (Wiener, 1954).

It is one thing to extrapolate the concepts derived from a field like electronic feedback systems and servo-mechanisms to another (human behavior), and to make analogies. It is quite another matter to put these notions to practical use. In making analogies, one points out similarities between properties or relations without identity. "When analogous systems are found to exist, measurements or other observations made on one of these systems may be used to predict the behavior of the others. The systems need not necessarily be analogous in every respect, but only in those respects which are of interest" (Soroka, 1954). In this paper the analogies I am pointing out between mechanical or electronic feedback systems and a social system are developed on the basis of similarities in gross functional properties. None of the reasoning by analogy of the feedback theorist will be of any consequence in the real world unless one has a certain amount of specific information. A crucial question is: What are the specific variables between which feedback relationships exist in a social system? If such knowledge is available, and if at least some of the relevant variables can be experimentally manipulated, one can proceed several steps beyond simply making intriguing analogies.

If some of the relevant variables in a system are known and can be manipulated, we are enabled to use feedback principles to do two things: 1) make the system operate faster or more efficiently; and 2) use novel, initially non-existent feedback arrangements as tools for the further experimental analysis of the system in question.

To make the above statements more concrete, let me illustrate with an example of an area of psychological research in which feedback concepts have been used to do exactly these two things. The area of research I refer to is engineering psychology as developed by P. Fitts, the late F. V. Taylor, and many others.

The basic ideas concerning the system approach of the engineering psychologist have recently been summarized by F. V. Taylor (1960).

The engineering psychologist has so far concerned himself with man-machine systems, such as the pilot in an airplane and the human operator in the control tower of a submarine. In the system approach to situations in which a human operator and a mechanical and/or electronic machine are interacting, the human operator is treated as a component of the total system. The first step in analysing a manmachine system is to identify the components of the system in terms

of their various functions. The other steps have been summarized by Taylor as follows:

- 1. One must "consider the engineering capacities and limitations of the pilot. For example, it is important to know the human's bandwidth in order to avoid demanding too much of the man. Also, the human's precision in carrying out operational transformations such as integration, differentiation, analog addition must be evaluated."
- 2. Once all "system-relevant characteristics of the pilot have been examined—. . . .—the next step is to consider the mathematics of the pilot's task." Taylor shows a schematic diagram of a pilot in a feedback loop where the "dynamics consist of two cascaded integrators. These dynamics are something like those of an airplane. The pilot looks at displays (D) and responds by applying force to his controls (C) in the effort to take out wind gusts which constitute the input.

"Controlling through two integrators is difficult. Due to the physics of the situation, there is a tendency for such a second-order system to oscillate or hunt from side to side. To stop this oscillation, the pilot actually has to carry out the mathematical processes shown; that is, he has to supply an amplification, two differentiations, and two analog additions. The box labeled T represents his reaction time."

3. "Having looked at the pilot's engineering properties and having determined the mathematical requirements of the system, the final step is to design the electronics and mechanics so that when all the components, human and non-human, perform their functions properly, system requirements will be met."

One way to improve the efficiency of a system is by means of "quickening" which may involve the shifting of various functions from one component of a system to another. "Although quickening accomplishes several different things, two effects are easily noted. First of all, the differentiations and analog additions are effectively shifted from the pilot into the dynamics. In this way, accurate and relatively noise-free computing circuits are substituted for the low precision, analog mathematics of the pilot. Second, in shifting the bulk of the computation onto the electronic equipment, the pilot is left free to devote all of his bandwidth and other resources to the simple job of amplification which remains" (Taylor, 1960, pp. 646-647).

There are no obvious reasons why the system approach described by Taylor cannot be applied to the social systems I have mentioned above. Is there anything which prevents us from transferring the manmachine system approach derived from the continuous feedback systems of the servo-mechanism engineers to social psychology? The research in the field of operant behavior, in addition to the existing management literature, as well as sociology, economy and anthropology (see Argyris, 1957; Bass, 1960; Katz, 1960, for many references to relevant literature), has provided us with extensive knowledge concerning the variables that control behavior. Servo-mechanism and system theory provide us with a partial language to describe complex social systems. Let us explore how a combination of the two may possibly lead to a truly experimental social science.

Several authors have suggested that "a cybernetics (or feedback) model is the best model for controlling an organization, be it a business organization or a state"3 (Bauer, 1960). One of the reasons that have prevented the servo-mechanism theorist as well as social scientists in general from applying system theory to social relations has been a lack of information concerning the manipulable variables in these situations. There has been no lack of criticism of current management practices and the mechanistic (Katz, 1960) philosophies behind them. Argyris (1957), one of the more outspoken critics, has given a clear statement of the differences in viewpoint between the "formal organizers" like Urwick (1944) and the many behavioral scientists and others whose views imply the recognition of the existence of what I have labelled feedback relationships (Ashley Montague, 1949; Bogdanov, 1913; Cooley, 1918; McDougall, 1923; Maslow, 1954; K. W. Deutsch, 1951; Newsom, et al., 1959; Skinner, 1953; and Wiener, 1954). But although many voice criticisms, few have put forward specific suggestions for a workable experimental approach to institute the changes envisaged by these new philosophies.

What kind of steps can be taken to put a systems approach into practice in the world of men and factories?

My example will concern itself with the situation existing in a factory for explanatory purposes only. Most of the suggestions will be applicable to other social systems.

The people who are involved in operating a factory (or a department within a factory) can be treated as a system involving two components or subgroups: the supervisors and the employees. That a functional distinction exists beween these subgroups has long been generally recognized. This distinction depends on a fact so well known that it is generally assumed as self-evident. The main difference between the two factions is usually ascribed to a conflict of interests which presumably has been the main force behind the rise of labor unions. In spite of many doubts and protests to the contrary (see Katz, 1960), management is mainly interested in increased profits. It has to be if it is not to be replaced by the board of directors or a more realistic enterprise. Increase of profits is partially attempted by maximizing production. The key problem here is to find the means of getting the

^{3.} One of the first to conceive explicitly of a systems type feedback arrangement in a social system has been B. F. Skinner. The notion is described in his novel "Walden Two." The feedback ideas contained in this book were the starting point of my own thoughts concerning the applications of operant techniques in industry and other social systems.

largest possible production output by the least costly possible means within the shortest possible time. 4

The main problem of each individual employee is quite different. He is not mainly interested in production or company profits in spite of recent attempts with contingent compensation plans. His problem is to find the means of getting the largest possible salary, the maximum amount of prestige and power, and the most favorable work conditions with the least possible amount of effort and time on his part. He is limited by certain minimum company requirements and his own conscience.

It has been obvious for a long time that these groups work in totally opposite directions: management tries to increase production, the employees in effect tend to pull it down. Any existing production rate of any plant is a resultant of these two sets of opposing variables. "The behavior patterns of employees and subordinate managers tend to stabilize and become routinized at the minimum level of performance which management is willing to settle for—balanced against some minimum level of job satisfaction which is tolerable to the employees" (Katz, 1960, p. 84).

How can this conflict be more efficiently resolved? Can job satisfaction as well as performance and tolerance of on-the-job work conditions be more objectively recorded and measured?

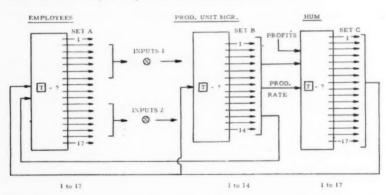


Figure 1. Schematic representation of some of the important feedback relationships that obtain between the various components of an employee-management system.

Suppose one pictures the head of a small production department as being in somewhat the same position as the pilot of Taylor's aircraft (Figure 1)⁵. A pilot's main reference variable is the degree to which

^{4.} Whether the maximizing of production is a desirable goal for society as a whole is a separate issue, which involves political questions that are not relevant to the present discussion.

^{5.} For purposes of simplification, the example discussed is a non-union shop.

his plane is off course or off target. The main reference variable of the head of a production unit is the production rate. On the basis of changes in this variable the department head or group leader may do two things: 1) report any unusually large changes (up or down) to higher-up management; 2) employ whatever means of control he has to readjust the production rate to its desired level.

In the case of a change upward, he may order a slowdown if overproduction threatens. If the increased production is welcome, he may praise his staff or crew, report favorably on them to higher-up management⁶, etc.

When the production rate is seriously decreased, he may try by whatever means of control he has to increase the output. Depending on his authority, which differs widely in various industries, he may threaten, cajole, fire employees, etc.

In actual practice, production rate is, of course, far from being the only reference variable a department head observes. A more complete list is given under Set A on the left side of Table 1 as variables 1 through 14. Not all of these events are actually observed in present-day practices. It is even more rare that they are actually being tracked or recorded. The first main point I wish to make here is that they could be. The variables listed as set A are probably not the only ones that could be objectively recorded. Any person with a detailed knowledge of actual factory conditions can suggest others (Baehr and Renck, 1958; Sillitoe, 1958). The present set is listed only as an example.

It is true that some of the events listed under set A in Table 1 are actually recorded, especially in larger organizations. This information, however, is rarely broken down for individuals or production units and returned to the units manager. If it is, this feedback is usually much delayed.

The center panel of Table 1 lists, under Set B, the "control variable" frequently used by a production unit or department head. By these means he may try to influence the behavior of the individual members of his crew—and he sometimes succeeds.

The production unit head reports to higher-up management (HUM). If the "means," listed as the variables of Set B, do not do the job of stabilizing the production rate, the production unit head may take his problem to HUM, which has additional, usually more powerful, variables at its control to increase or decrease production by influencing the behavior of the production crews. Such variables are listed under Set C of Table 1.

^{6. &}quot;Higher-up management" will hereafter be referred to as HUM. Any similarity between this term and the term noise in a feedback amplifier system is purely accidental.

^{7. &}quot;A variable is a measurable quantity which at every instant has a definite numerical value." If there is any doubt whether a particular quantity may be admitted as a variable . . . use the criterion whether it can be represented by a pointer on a dial" (Ashby, 1952, p. 14).

TABLE 1

-	LIST OF THREE SETS OF POTENTIAL VARIABLES ASSOCIATED WITH THE SOCIAL SYSTEM OF A SMALL INDUSTRIAL PRODUCTION UNIT	ABL	SMALL INDUSTRIAL PRODUCTION UNIT	S S	JAL SYSTEM OF A
	Ser A		Ser B	_	SET C
_	frequency of complaints (grievance committee)	П	praise, recognition	1	salaries, wages
01	cleanliness of plant	01	special privileges	61	wage incentives
က	insults (frequency)	က	promotions	9	Christmas bonus
4	work slow down (production rate)	4	job assignments	4	cost-of-living bonus
10	number of coffee breaks	70	vacation permission	70	profit sharing
9	length of coffee breaks (minutes)	9	overtime permission	9	extra holidays
1	sickness (hours)	1	call-in, report-in pay	1	vacation length
00	absenteeism (hours)	00	transfers	00	hours of work
6	number accidents	6	progress report	6	leave of absence
10	number latenesses	10	discipline	10	exhortations
11	time lost due to lateness (minutes)	11	reprimands	11	company propagand
12	excessive lunch time (minutes)	12	threat of discharge	12	insurance
13	visits to washrooms (number)	13	probation	13	credit union
14	time lost in washrooms (minutes)	14	layoff	14	seniority privileges
15	requests for transfer			15	retirement plan
16				16	discharge
17	number of objects (company property) which disappear from the premises (weeks)			17	severance pay
				-	

Figure 1 shows some of the important feedback-type relationships that exist between the components of the E-M system. In this flow chart or block diagram, HUM has been included as a third component. The diagram may be viewed as an oversimplified representation of a "chain of command." The kinds and direction of the interactions between the initial links of the E-M chain are indicated, however.

How can the over-all performance of this system be improved? Can the system be "quickened," like a radar gunfire control system, by transferring functions from one component to another?

One of the important factors that make any kind of closed-loop feedback control system function more adequately in terms of stabilizing its main reference-variable (s) is the speed with which the feedback loops adjust to changes in the variables generated by the subsystems. Taylor (1960) has suggested that "the presence or absence of a tight feedback loop is the key to the puzzle as to which of the undertakings of human engineering have benefited most from the system viewpoint." It might, therefore, be suggested that speeding up feedback where no "tight" feedback loops exist may improve the functioning of a system.

In the E-M system speeding up feedback can take many forms. One approach would be to improve the "displays" (indicators) on the basis of which the various subgroups make their decisions. It has already been pointed out that most of the events listed as Set A in Table 1 could be objectively recorded.

Some of the variables clearly indicate the degree of dissatisfaction of the employees with their immediate supervisors and HUM. Others, like sickness, are confounded with other factors. A daily record of all these events would, however, provide both the first line supervisor and HUM with very useful information. This information would also be useful for an employee or union representative. An objective record of the degree of employee dissatisfaction might go a long way towards eliminating the mistake on the part of a union, in which a strike is called and subsequently fails because of the absence of a sufficient number of grievances on the part of the rank and file.

A more revolutionary approach to quickening involves the redistribution of functions between the various components of the overall system. This approach has been found highly effective in manmachine systems (Taylor, 1960).

To do so most effectively will involve the introduction of computors to take over some of the functions and calculations now performed by certain employees. One example of the use of computors here might be in speeding up the calculation of a bonus under any of the presently employed wage-incentive plans.

One important fault of most of the currently used incentive plans (see Sibson, 1960, for a general survey) is that the actual gains in wages contingent upon increased production are not directly and immediately contingent upon performance. The exception here is the simple piecework plans in which the workers' earnings are directly and immediately related to his productive output. But even here many and long delays are involved between the actual time the work is done and the time of payment of the bonus. One of the outstanding facts about behavior shown by extensive experimentation with both humans and animals is that: 1) the effectiveness of a reward drops precipitously with any increase in the delay between performance and reward, and 2) that the effective range of action of this delay factor is a matter of seconds if not milliseconds. It has also been established (experimentally, but mainly with animals) that the decreased effectiveness of a delayed reward can be partially cancelled by bridging the delay period with intermediate token or symbolic rewards.

Each person on a production line, for example, might be given an indicator, such as a gauge or counter, which immediately informs him concerning 1) his production output and/or 2) his bonus as contingent upon his output. In this way the calculations of a bonus is taken over by a computor that immediately feeds its information back to the individual worker. An additional benefit of the introduction of such a system, besides increased production output and worker morale, would be the amount of time saved in computing bonuses. This latter activity at present involves many individuals in an organization8. Further specifics concerning the immediate ways in which such an approach could be tried, and if necessary be made more sophisticated, have no place here; dozens of major and minor variations could be suggested. The main points of the above example are the following: no matter which approach is tried, its effectiveness should and can be judged by two objective criteria-1) does it increase production, and perhaps more important 2) does it decrease social friction or employee dissatisfaction as gauged by such variables as the ones listed under Set A, Table 1.

The success and effectiveness of any change brought about for experimental purposes can thus be evaluated. A successful system from everybody's point of view is one which reaches an equilibrium point at which social friction is minimized for the largest possible production rate. The above sentences may remind some readers of a paper by Harry A. Hopf (1935) entitled "Management and the Optimum." The editor's introduction to this article, which was recently reprinted (Merrill, 1960, p. 355) reads as follows:

⁸ Many industrial organizations have somehow gotten into the peculiar situation in which a large percentage of its members do nothing but record with paper and pencil what the rest does. This situation is possibly not unlike the one existant in several political bureaucracies. Parkinson (1957) has recently proposed several generalizations that give a preliminary explanation of these phenomena.

It is the thesis of this paper that the time is ripe for transformation of the science of management into a new and much more inclusive science—optimology, the science of the optimum. The author argues that the practice of management has reached its fruition in the creation of vast combinations of men, methods, and money which must inevitably defeat their own ends and lead to ultimate disaster. By numerous example from his practice as a management engineer, he shows how, by failure to achieve and maintain optimal conditions, many business enterprises have sustained losses or met failure.

Defining the optimum as that state of development of a business enterprise which tends to perpetuate an equilibrium among the factors of size, cost, and human capacity and thus to promote in the highest degree regular realization of the business objectives, the author describes how, in the field of life insurance, he has made extensive measurements of business results and managerial capacity and established optimal areas of operation for ten large companies. He suggests that, by the application of similar techniques, the optimum can and should be ascertained for every business enterprise.

The techniques of 1) redistributing functions between components of a system, and 2) adding new previously non-existent feedbacks can be further explored.

A far more radical approach to solving the "behavioral engineering" problems discussed above is implied by the following suggestions.

Every industrial organization is faced with problems of wage and salary administration. During the last decades these problems have come to be solved by a series of formal and increasingly more sophisticated ways (Sibson, 1960). In essence they all involve the steps listed in Table 2.

It is widely recognized that, at present, there are no objective criteria, and consequently there is no conclusive research, to determine the value, efficiency, validity and reliability of the various job evaluation, grading and pricing procedures. Sibson (1960) has stated that "empirical tests (to determine the accuracy of merit rating systems) will probably never be made because of the virtual impossibility of setting standards for measuring accuracy. There is little doubt, however, that even the most formalized merit-rating program is a highly subjective process. Human judgments based upon only a partial knowledge of the quantities involved (performance, potential, and personal

characteristics), as measured against a very imperfect yardstick, are bound to yield results which are in themselves far from perfect" (Sibson, 1960, p. 95).

Is the situation actually so hopeless? What alternatives are available to the interview techniques, questionnaires and other means of making inferences concerning behavior developed by social scientists during the last decades?

Could we take seriously the age-old dictum that what a man does is more important than what he says (about why he does it)? If, as it has been suggested (Argyris, 1957), it does not do to tell people what they should do and be motivated by, how can we find out what it is that people are motivated by?

One approach might be to extend the basic idea behind the voting machine⁹ to other walks of life.

TABLE 2
ELEMENTS OF A FORMAL WAGE AND SALARY PROGRAM¹0

Technique Used	Objective of Technique			
Job analysis	To determine job facts as a necessary step in job evaluation (as well as in employment, management development, organizational studies, and general supervision)			
Job description	To record job facts			
Job evaluation	To determine relative job worth			
Job grading	To determine pay scales effectively			
Job pricing	To translate relative job worth into money values.			
Incentive or merit plans	To reward employees for higher production and better job performance.			

Figure 2 is a deliberately oversimplified representation of a scheme for a novel approach to some of the questions raised above: several problems are solved by eliminating them.

Part of the job of the "first-line supervisor" has been turned over to a computor. The employees as a sub-component of a total system are in a feedback relationship with the computor. There are only two

^{9. &}quot;pulling levers in private"

^{10.} Adapted from Sibson, 1960, page 21.

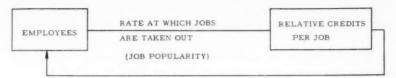


Figure 2. Simplified representation of a novel feedback interaction that employes credit shifting.

variables in the system: 1) The computor monitors the rate at which jobs are taken out by the employees. 2) The computor adjusts the relative credits per job on the basis of changes in item 1 above. Job popularity or desirability is thus controlled by varying the relative credits for a job. This device will be called "credit shifting."

The system is based on the assumption that the following conditions are met:

- 1. A computor is available in case of a large-scale application involving many workers. (A computor is unnecessary in a small-scale application.)
- 2) A large number of the jobs in the system are interchangeable. Interchangeable jobs are defined as those jobs which all members of the "employee-component" can perform without any special or extended training. The specific nature of the interchangeable jobs therefore depends on the nature of the work force involved. The jobs may consist of purely manual tasks in the case of unskilled labor. They may be jobs requiring special skills and technical knowledge in the case of white-collar personnel.

In addition to the general features described above, the following conditions should be met:

- 1) The members of the system-component are permitted to choose from the pool of all momentarily available interchangeable jobs on a first come, first serve basis.
- 2) Each employee chooses in private. The reason for this is identical to that at a political poll—elimination of intimidation by others.
- 3) Each job has a temporary relative credit rating, which varies in time.
 - 4) The relative credits are eventually interchangeable for money.
- 5) All employees are told about and should know all the features of the system and the rationale for each one.

Several other features can be tailored to the individual requirements of each specific situation (production line, service department, secretarial pool, etc).

- Jobs might be chosen either for a certain length of time or as a piecework unit or a combination of these two basic alternatives.
- 2) Employees may or may not be informed how much of each job is available. Ignorance concerning specific job qualities outstanding eliminates scheming; for example, by trying to beat the system by stalling temporarily on momentarily poorly paid unpopular jobs.
 - 3) The time permitted to choose might be restricted.
- 4) In a more sophisticated and complex system each job might be given a "bonus credit" that varies with the urgency of the job and time of day. In most present-day situations night jobs usually pay more.
- 5) Work time may or may not be restricted. In certain situations it might become feasible that employees can come and leave any time within wide limits.

The above system can be generally compared to an auction system adapted to an industrial situation. The difference between the present system and the regular auction system is the feedback scheme of the former-the auctioneer can never lower his price, the computor does. Furthermore, the auctioneer does not give an explicit choice between alternatives but sells one item at a time with or without displaying all of his wares. In the system described above, employees and management represented by a computor are put explicitly into a continuous interacting relationship in which they continually adjust to changes in each other's behavior. The system is perhaps more like the form of social interaction which permits haggling on the Paris flea-market; however, the participants do not contend over trifles! This system does make explicit already existing situations—that any employer buys the time and labor of his employees and that unpopular jobs take more money (credits) to be "sold." In essence the employees are repeatedly asked what jobs they like best (or dislike least) and how much (in credits or money) is necessary to persuade them to do them.

The basic simplicity of the above scheme which uses credit-shifting as the only *immediate* feedback device to insure that all necessary jobs be done permits several other innovations with respect to specific work conditions.

The use of negative incentives in the form of discipline and reprimands is eliminated. Furthermore, positive incentives can be obtained frequently (after completion of each job-unit) and be provided immediately.

In any large-scale trial of the workability of the above scheme several other features will have to be included. Several important ones may be listed as follows:

- 1) Looking for jobs to be done and reporting them to the jobinventory file can be made one of the jobs in the interchangeable job pool.
- 2) Inspection whether a job has been done can be made a part of interchangeable job pool.
- 3) The person who looks for and reports jobs can at a later time choose to do those jobs. However, he *cannot* inspect his own jobs as to whether it has been done and completed.
- 4) Any job chosen may become automatically a job to be inspected as to whether it has been done.

If a large-scale system like the one described is to be put into practice, the computor used should be able to 1) keep up several running inventories, and 2) continuously cross-check between several of these inventories. Basic inventories are as follows: 1) outstanding jobs, 2) credits earned by each employee, 3) jobs completed, 4) jobs to be inspected, and 5) frequencies with which specific jobs are taken out.

The computor may furthermore keep a record of the day-by-day job history of each employee—what, when, where, and how often. Cross checks between various inventories are to be made continuously. Every time a new job is taken out of the system, the job-credit ratings may have to be changed. In a complex system, distinctions might be made between various kinds of employees in terms of age and health. A man with a heart condition should not be tempted to lift crates because of a momentary high relative credit rating. With such features added, any job chosen by an employee has to be instantly checked against an employee's job qualification for example, by means of a code number).

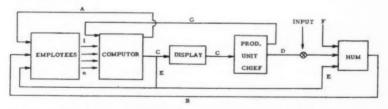


Figure 3. Schematic representation of a feedback arrangement that involves credit shifting within the total organization of an employee-management system. The meaning of the symbols is as follows: 1 - N = Set A variables; A = relative credit values (points); B = a absolute credit values; C = "social friction" + production etc.; D = information from unit chief to HUM; E = production etc.; F = profits; G = computor adjustments by unit chief.

The system has certain obvious advantages both for the employees and management. Management has available a continuous objective performance record of the employees and how each individual performs with the total system. Whether a credit-shifting system will in practice increase employee productivity cannot be predicted in ad-

vance of trying it out. It should be reasonable to expect such a result, since the plan has certain inherent features which have been widely recommended by industrial relations experts:

- 1) A fuller, if not maximum, use is made of individual employee capabilities.
 - 2) Job monotony is considerably decreased.
- Inter-employee rivalry is reduced to a minimum, gold-bricking is eliminated.
- 4) The plan insures fair criteria by which employees are reimbursed for their time and efforts. Each person has the same opportunities in choosing work.

Whether a novel approach as the one outlined above will be accepted depends, among others, on two factors. The degree of acceptance of a new social scheme is inversely related to 1) the number of people it is likely to depose, and 2) the power positions of these individuals. It may, therefore, in conclusion be pointed out that nobody is of necessity replaced by a computor.

Figure 3 shows how a scheme of the type elaborated might function within many existing industrial organizations. The computor which performs any and all of the functions described previously is "inserted" between the employees and the first-line supervisor or production-unit chief. Several feedback arrangements consistent with existing practices have been included. The previous discussion made no mention of the ways in which the "relative credits" would be transformed into hard cash. This perogative is, in Figure 3, left to HUM. This variable as well as many other work conditions are still left to be bargained over by employees and/or unions on one side and management on the other side of a solid table. Feedback loop "g" involves a suggestion that the unit chief (together with a union representative) can adjust the activities of the computor if necessary. In an ideal situation, the computor merely serves as an aid to both management and employees.

In conclusion to the above, I have not found it possible to write my own epilogue. The following quote, however, seems relevant:

We are today in a period when the development of theory within the social sciences will permit innovations which are at present inconceivable. Among these will be dramatic changes in the organization and management of economic enterprise. The capacities of the average human being for creativity, for growth, for collaboration, for productivity (in the full sense of the term) are far greater than we yet have recognized. If we don't destroy life on this planet before we discover how to make it possible

I believe that the industrial enterprise is a microcosm within which some of the most basic of these social changes will be invented and tested and refined. As Peter Drucker has pointed out, the modern, large, industrial enterprise is itself a social invention of great historical importance. Unfortunately, it is already obsolete. In its present form it is simply not an adequate means for meeting the future economic requirements of society. The fundamental difficulty is that we have not yet learned enough about organizing and managing the human resources of enterprise. Fortunately, an increasing number of managers recognize the inadequacy of present methods. In this recognition lies the hope of the future. Industrial management has again and again demonstrated an amazing ability to innovate once it is persuaded of the opportunity to do so (McGregor, 1960, pp. 244-245).

SUMMARY

An employee-management system is described which is explicitly based on feedback principles. The system as outlined is essentially a proposal for mutual employee-management experimentation. Specific details necessary for its successful functioning are presently unknown and will have to be determined by putting it into practice.

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SCALING THE ACCURACY OF RECALL OF STORIES IN THE ABSENCE OF OBJECTIVE CRITERIA¹

DAVID J. KING

The American University

In two previous studies (King, 1960; King and Schultz, 1960) a method of evaluating various methods of scoring the accuracy of written recall were presented. Briefly, this consisted of utilizing the method of rank order to scale the recalls of stories for accuracy of recall and thus obtain scale values to use as criteria. In the scaling procedure, twenty judges ranked fifteen recalls of each group in order of increasing approximation to the original story, the original being placed in front of them. The recalls of the twelve groups studied were also scored for accuracy of recall by six other scoring methods. A seven by seven correlation matrix was obtained for the recalls of each group and these matrices were then factor analyzed. All matrices revealed two factors that accounted for nearly all the variability. These factors were identified as length (simply the total number of words) and a second factor, the nature of which varied as a complex function of the subject material of the story and the degree of learning.

The present research utilized the recalls of four of the above mentioned groups. The recalls used were those of groups A and B who had a story called Who Shall Go read to them once or twice before recall and those of groups C and D who had The War of the Ghosts read to them once or twice before recall. The hypothesis of this experiment was that the recalls could be scaled for accuracy by a variation of the method of single stimuli as applied to ranking, i.e., that the judges could reliably rank the recalls of stories for accuracy without any knowledge on the part of the judges of the original stories. If this were possible, it was further hypothesized that the resultant criterion scores would have a relatively larger proportion of their predictable variance accounted for by length than by content words as contrasted to the relative proportion predicted by length using the traditional method (wherein the original stories are used by the judges as criteria). In the first study (King, 1960), the relative contribution of total number of words was much greater than content words in the recalls of groups A and B and, although to a lesser degree, the relative importance was reversed in the recalls of groups C and D. Thus, in the present study, one would expect, assuming that scaling is possible,

This investigation was supported by a research grant M-3174(A) from the National Institute of Mental Health, United States Public Health Service. The author is indebted to Profs. Charles N. Cofer of New York University and James Deese of The John Hopkins University for their interest and for their critical reading of the manuscript.

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that the relative importance of number of words and content words would be reversed in groups C and D.

PROCEDURES

Each set of recalls was scaled according to the method of rank order in a manner identical to that previously described (King, 1960) except that the judges were not given the original stories to use as criteria for accuracy. The judges (undergraduates) were told to try to imagine what the original story was like from the recalls and to set up a standard on that basis. Each judge ranked all four sets of recalls and the order in which the stories were ranked were varied from judge to judge. Upon questioning, none of the twenty judges said that they had read or heard either story prior to the experiment.

Scale values and their reliabilities were calculated by the same procedure as in the previously cited studies.² The reliabilities of the scale values under the present procedure were .944, .871, .863 and .866 for groups A through D respectively. Although the reliabilities of the scale values are somewhat lower with the single stimuli variation method, there can still be little doubt that the judges are able consistently to rank the recalls of stories without knowledge of the original story.

The correlations between the criterion scale values and the six other scoring methods were also computed. There was very little difference between standard scale scores and scale scores obtained by the present method in the correlation of the other scoring methods with the criterion scores for the recalls of groups A and B. In the recalls of groups C and D there were three consistent changes when the criterion scores were obtained by the present method as contrasted to the regular method. There was a decrease in the correlation between the cloze procedure and criterion, a decrease in the correlation between the number of content words and criterion, and an increase in the correlation between the total number of words and the criterion. The changes in correlation of the number of content words and the total number of words were expected. With the judges being asked to rank a series of recalls for accuracy without the original story being present, they would have to pay less attention to specific words and more to length. The importance of content words would not completely disappear for two reasons: first, number of content words and total number of words are correlated and secondly, the judges could infer some of the content words from the consistent appearance of words in many of the recalls. The drop in the cloze procedure score also seems reasonable as the words eliminated in the cloze procedure were always content words.

As a further illustration of the decreased importance of the

^{2.} It might be noted that the reliabilities in the present situation may also be determined by a technique developed by Horst (1949). The reliabilities calculated by the Horst technique are nearly identical to the values obtained under the present procedure.

number of content words and the increased importance of the total number of words in the recalls of groups C and D, the relative contribution of both variables in predicting the criterion (scale scores) was determined. This was done by multiplying the Beta weights, B15.6 and B16.5 by r15 and r16 respectively. The products indicate the relative contribution of the two independent variables to the total predicted variance of the criterion. Table 1 presents the results of this analysis for both the present (V) and the previous (S) research which utilized the traditional rank order method of scaling. The only clear and consistent change is the predicted one, that is to

TABLE 1
THE RELATIVE CONTRIBUTION OF THE TWO INDEPENDENT VARIABLES TO THE CRITERION SCALE VALUES

Content Words			Total Number of Words	
Group	S	V	S	V
A	.091	.133	.624	.682
В	.158	.199	.575	.463
C	.557	.081	.309	.619
D	.507	.152	.328	.728

say, the reversal of the relative predictive importance of content words and total number of words in the recalls of groups C and D.

DISCUSSION AND CONCLUSIONS

It appears to be possible for judges reliably to rank the recalls of stories for accuracy of recall without the original stories being present to use as criteria. The basis upon which these judgments are made seems to be primarily, although not entirely, the length of the recalls. In two sets of recalls (where, utilizing a scaling procedure that included the use of original stories as criteria, the relative importance of content words was greater than total number of words for predicting the scale values), the relative importance of these two independent variables was reversed when the present method of scaling was used.

Finally, it should be noted that the scaling procedure utilized in this research is perhaps not properly described as a variation of the method of single stimuli. As all fifteen recalls in a group are available for simultaneous inspection a more precise term might be concomitant stimuli. In any case, the relation of this technique to Helson's adaptation level (Helson, 1947; Helson, 1948) is obvious.

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PERSPECTIVES IN PSYCHOLOGY XVI. NEGATIVE FINDINGS

IRVIN S. WOLF

Denison University

The logic associated with testing the null hypothesis is deceptively appealing. Every beginning class in statistics seems to be intrigued and persuaded by the example of being able to disprove, but not to prove, that all crows are black. The impossibility of proving the proposition is presented as the timeless problem of induction—when may we be confident our observations are sufficiently extensive and accurate to escape tentativeness, to exclude the possibility of the negative case?

The compelling arguments for following this interpretive device are accompanied by problems neglected by slaves to its use. Involved are situations both where the null hypothesis is rejected and where it is accepted. In the former we are sometimes too ready to conclude with finality that the obtained difference is true, and dependent upon the chosen experimental variable. Overlooked are admonitions regarding limitations of statistical tools, which, no matter how elaborate, cannot be expected to correct for problems of control or observation. Perhaps it is the present aura of unguarded respect for quantification and all that is mathematical that leads to careless use of statistical procedures in our descriptive, investigative, and interpretive enterprise. Rejection of the null hypothesis occurs only at a particular level of confidence and requires assumptions of representativeness of sample and absence of constant errors built into the design Interpretations based on rejection of the null or measurements. hypothesis also must remain tentative. To make significance at the .05 level the criterion of acceptability as a legitimate conclusion gives a statistical rule the status of final arbiter over the admissability of scientific evidence. We need to be on guard against this species of absolutism, overconfidence in reaching truth by a single route, statistical or otherwise. Publishing patterns at least suggest such overconfidence exists. Rejecting the null hypothesis seems almost an end in itself. Perhaps there is a basis for the charge that in some of our journals there is little more than psychological "trivia" dignified by impressive exercises in experimental or statistical design. worthwhileness of method alone cannot determine the worthwhileness of the result.

The Problem

Among the present trends one which should cause concern is that of minimal attention to negative findings; few studies are reported where the null hypothesis was not rejected. An editor of this journal once warned that accepting a particular manuscript might lead to our becoming "typed" for publishing negative findings. That these should be available and are not has been emphasized (Goldfried and Waters, 1959; Wolins, 1959; Sterling, 1959). The latter reviewed a single volume of each of four psychological journals and found that of the 362 papers none was a replication of a previously published paper. Tests of significance were employed in 294. Only eight of these reported failure to reject the null hypothesis when attention was focused on the study's major issue. Sterling observed further that this situation could result either from editorial or author decision or both.

Uses of Negative Findings

Negative findings should be considered for communication to colleagues; such pieces of research with some theoretical or practical import, competently studied and interpreted, should be accepted in the literature. Despite apparent reluctance to report negative findings, their recognition may prove profitable in various ways: by directing attention to relationships which do *not* exist; by refining our knowledge of the factors which lead to conflicting positive and negative findings in studies with similar conceptual parentage; in drawing attention to repeated "near misses" (e.g., at the .10 level); in demonstrating the possibility of having rejected the null hypothesis in a previous study when it should have been accepted, etc.

Prevailing attitudes disparaging negative findings may lead a discouraged investigator prematurely into other areas. The least that can be gained from publication, if attainable, is that other workers may be directed to avoid treading the same inconclusive steps.

Comparison of similar studies with positive and negative findings may lead to more refined delimitations of significant variables.

Even a neutral reviewer of the literature may be handicapped where he has no basis for choice between two studies with differing theoretical orientations, each reporting positive findings perhaps with equal significance levels. The problem of choice probably could be resolved were he aware of negative findings in the replications of one. In time, of course, we might expect a preponderance of positive

^{1.} Each year there appear articles admonishing us with regard to uses and abuses of statistical tools. During the present manuscript's maturation in the file, several papers commenting on its topic have been discovered in recently appearing journals (Goldfried & Walters, 1959; Sterling, 1959; Wolins, 1959; McNemar, 1960; Rozeboom, 1960). It is not without some embarrassment that we have proceeded to publish these notes, but the fact that some of the points are not completely unshared perhaps attests to our sanity . . . and then nearly all those discussing the issues have at some point stressed the importance of "replications".

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findings to be reported for the other. Prior to that, wasteful theoretical and experimental decisions would be unavoidable.

Convinced of the significance of a particular negative finding one may seek, through a replication of the experiment or through collecting of other instances from the literature, to "confirm" the absence of rela-While greater confidence may inhere in building conceptually upon rejection of the null hypothesis, there is no logical basis for concluding that "no differences" or "no correlations" are non-existent in nature. Tests of hypotheses which are of sufficient theoretical or practical significance to lead to repeated observations by the same or different students but which always end in failure to reject the null hypothesis, begin to approximate "proof" that differences cannot be obtained under these conditions. Perhaps the dog is not capable of color vision but a more tentative statement would be that color discrimination has not been demonstrated. of the latter form has much in its favor despite the fact that we usually communicate the former. That the proper technique for demonstrating color vision in the dog has not been utilized must be considered a possibility; comparable caution in interpreting the positive findings of color vision in other species is also wise (e.g., the possibility of inadequate control of brightness or some other variable not now recognized). We need constant vigilance with both positive and negative findings.

A decision in proposition building by the serious scientist will be based on preceding studies, nearly identical, and related; yet the acceptability of evidence tends to be time-bound. Studies are evaluated as discrete, scientific episodes. The theory is supported (or not) on the basis of our success or failure in rejecting the null hypothesis—in this group of manipulations. Which is more acceptable—several instances of significance at the .10 level for repeated tests of one hypothesis or one "success" at the .05 level for another? While the former many times fails to reach consideration in our theory building, the latter achieves acceptance and even if in error resists correction simply because negative findings in future tests are not reported.

It is not inconceivable that a completely autistic theory, appealing perhaps because of its bizarreness (or the professional status of its author), would be tested a sufficient number of times so that results involving a Type I error would occur. Even though statistical significance was achieved on a chance basis, or because of some constant error (which statistics cannot be expected to correct), the studies carefully executed and reported may now become a part of the literature. The students of the investigator, particularly if he is a distinguished one, and their students now perform replications of the original experiment or deduce related research projects. Again out of the mass of studies some will produce positive findings and they too

WOLF

will find their way into the literature. Those failing to achieve statistically significant differences, or even those with results in the opposite direction (when the one-tailed test was pre-selected), will probably be rejected by the student himself and the experiment will be redesigned (doctoral committees as well as editors are reputed to prefer positive findings), With final "success" in achieving positive results the preliminary studies become known as "pilot" work; although the dissertation summarizes the whole investigative effort, the positive findings have a central position in the limited space available in our journals. Today it is not considered proper to shift one's statistical stance during the course of an investigation (e.g., changing from a one-tailed to two-tailed test). Perhaps with equal fervor we should insist upon presentation of negative findings for a study which, prior to its execution, was agreed to be a fair replication. Positive findings become established by being accepted and printed, but the set of attitudes discriminating against negative findings tends to protect a Type I error.

We could pursue the hypothetical history of a theory one step further. Still another student may now get his MA for counting the references to the author of the original papers. Although misgivings are expressed regarding the interpretations because of rumored failures to find consistent support, the theory will be glorified for its heuristic contributions!

Cautions

It should be made clear that, although the foregoing was offered as an indictment of present neglect of negative findings, all such results do not have equal status. Significance of the hypothesis being investigated, care in the execution of the study, consistency (and inconsistency) with results obtained in similar studies—all help to determine the evaluation of a particular negative finding. Not the least of the factors is how great was the "miss" (at the .06 or .40 level).² We must remain sensitive to our constructional procedures. Negative findings too, in a study with inadequate methodology or conceptualization, can be misleading. (How much has been attributed to instinct or constitutional factors because of the inability to demonstrate learning!).

Summary

Some of the dangers of translating rules for testing the null hypothesis into absolutistic criteria for the acceptability of scientific

^{2.} Eysenck (1960) points to the essentially "subjective" nature of the verbal descriptions, significant and non-significant, used in dichotomizing the continuous series of p values around arbitrary points of .05 or .01. He recommends abandoning this translation procedure (involving other terms like "almost significant" or "significant at the 10% level") and the implication of successful or unsuccessful research. Presenting the p values themselves would permit a reader to make an interpretation in terms of evaluations of this and related studies and of other factors. (Also, see Sterling (1960) pg. 30.)

evidence have been discussed. Rules so used take the place of events and other determiners of investigation molding the course of psychology. An attitude of tentativeness still needs to be maintained with positive findings. Replications are to be encouraged. Negative findings provide information and need a more favorable audience among psychologists—if only for the reason that prejudices against their publication (by both editors and investigators) tend to perpetuate a false conclusion based upon rejection of the null hypothesis when it should have been accepted. Judicious consideration and communication of negative findings seem necessary for the development of our science.

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The Psychological Record, 11, 1961, 96.

AMERICAN PHYSIOLOGICAL SOCIETY. Handbook of Physiology: Section 1: Neurophysiology. Three volumes. Baltimore: Williams & Wilkins, 1959, 1960. Pp. 2013.

These volumes constitute Section 1: Neurophysiology, in what the American Physiological Society is presenting gradually as a multi-volume Handbook of Physiology to "provide a comprehensive survey of the experimental findings and the concepts which constitute the substance of present-day physiology." The Society intends "to cover the physiological sciences in their entirety once in about ten years, and to repeat the process periodically thereafter."

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This "review" is presented more with awe than with a claim to comprehensive understanding—and only with selected reading. The primary purpose is to bring the material to the attention of psychologists. Much will interest many, and a number of chapters are written by men best known as psychologists. The importance of some of the summaries for our field depends on one's theoretical orientation.

Like Bartley and Nelson (this journal, 1960, p. 319) in reviewing the currently appearing work of Koch for the American Psychological Association, we are led to comparisons. Like them, we see the efforts of the physiologists more in the tradition of the *Handbuch*. While the volumes on psychology are to date organized more around theories, the treatments of neurophysiology are divided into investigative areas. Theory appears as part of the body of material and does not dominate the presentation. From these projects of their respective associations, one might obtain the impression that psychologists have been spending most of their time speculating while physiologists have been observing. The American Physiological Society is to be congratulated for the objectives and progress of this project. The results are impressive and you are urged to examine them. We psychologists might find a similar project worthwhile.

The Psychological Record, 11, 1961, 97-99.

UNDERWOOD, B. J. and SCHULZ, R. W. Meaningfulness and verbal learning. Chicago: Lippincott, 1960, Pp vi + 430.

In their first paragraph, Underwood and Schulz invoke the name of Ebbinghaus, and an appropriate reference it is too, for in subject matter, in technique, in the careful experimental data and in importance, though necessarily not in originality, the present work is quite comparable to *Ueber das Gedächtnis*.

In brief, this book summarizes the available literature and reports a number of original experiments concerned with some independent variables, which the authors group together under the rubric *Meaningfulness*, and their effects in rote learning. The meaningfulness concept springs out of the traditional notion of association value, though the authors believe, in the end, that it points to the more fundamental variable of frequency. In their very last page, the authors say that they are defending and promoting the use of the two classical associationistic variables, *frequency and recency*. This is a welcome relief from the long and usually empty list of so-called secondary principles of association that one finds from Thomas Brown to E. S. Robinson.

The authors justify the use of a single concept of meaningfulness to describe a number of measureable variables because of the high intercorrelation between these variables. These include the number of associates produced to a stimulus word in a fixed period of time, whether or not an associate occurs at all, ratings of familiarity, ease of learning and pronounceability (the latter turns out to be the best predictor). Thus meaningfulness is an intervening variable in the fullest sense, though a critic might ask why Underwood and Schulz did not factor their variables to find out just how much variance was attributable to a single factor and just how a second factor might have been loaded on these variables.

The basic strategy in the research reported in this book consists of the study of correlation (though the correlation coefficient itself is not always used) between these variables (with items serving as "individuals") and measures of performance in various rote verbal learning situations. Sometimes the magnitude of correlation coefficients under different conditions turns out to be the dependent variable. At any rate, the research is limited by the kinds of inference that can be drawn from correlation studies. This is not intended to be a critical remark, since this reviewer is convinced that the correlational approach is the most fruitful strategy for research in language at present, but the reader needs to be reminded of the inherent limitations of the technique in evaluating the conclusions drawn by Underwood and Schulz, conclusions which put much burden on frequency of experience as the source of the offects reported.

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The major part of the book is devoted to the relationships between frequency of experience (inferred from normative data) and what the authors rather inelegantly but aptly characterize as the spew hypothesis. The spew hypothesis says that the frequency with which verbal units have been experienced directly determines their availability as responses in new associative connections. The spew hypothesis is similar to but different from ideas presented by a number of other investigators of verbal behavior in recent years. The unique characteristic of the spew hypothesis is not the spew process itself (as one might infer from the name) but that the frequency of availability is determined by the frequency of experience. The notion of availability matching the response requirements in various kinds of verbal learning is a notion that occurs in the work of a number of investigators (including that of the present reviewer), but the notion of spew determined by frequency of experience makes the Underwood and Schulz hypothesis a much more precisely determined theoretical variable, though it may also be a notion of limited utility.

Another notion of considerable importance that runs through the book is the "stage" analysis of rote learning, an idea that occurs in earlier publications by these authors. Stated briefly, the stage analysis takes advantage of the facts that subjects in rote learning experiments must learn both to emit the responses required and to associate these responses with the correct stimuli. Conveniently, in the situations investigated by Underwood and Schulz these learning processes usually take place in order and separately. The spew hypothesis applies to the response learning stage, and it is to the analysis of this stage that most of the experimental work reported in this book is directed.

Not the least useful part of this monograph is provided by the appendices which consist of normative data on rote verbal learning materials. These are, so far as the reviewer knows, the most exhaustive and complete set of materials of this kind available, though similar and in part overlapping normative data are available from the recent work of Mandler, Noble and Archer. The old Glaze, Krueger and Witmer norms are presented, as are the 1952 Noble norms. In addition, Underwood and Schulz present frequencies of bigram and trigram letter combinations, both in the familiar Pratt count and in a new count from Thorndike-Lorge data, and they also present some very useful pronounceability ratings on monosyllables as well as some association frequencies (with letters as responses) to single and two letter combinations. All in all, this is a most useful collection of data.

The greatest methodological defect, in the eyes of this reviewer, is the great reliance on the nonsense syllable. This makes a very clumsy analysis necessary, since, as the Underwood and Schulz norms themselves demonstrate, nonsense syllables can vary from complete disconnectedness to complete linguistic encoding. This in itself provides a

useful dimension of variability in linguistic material, but Underwood and Schulz make the analysis difficult by using nonsense syllables in the traditional arrangements of three letters in N items. Thus, for many purposes it is indeterminate whether a twelve item list really consist of thirty six units or twelve units or something in between, determined by the transitional probabilities between letters and the trational mode of presentation (three letters per "response").

As to the grand strategy of the research reported, many of us will have reservations about the general importance of frequency of experience compared, say, with the role of grammar (both intra-linguistic and linguistic-object) in the determination of verbal behavior. It is possible, for example, that the sort of thing that lies at the heart of the Underwood and Schulz analysis, frequency counts, may be generated by grammars and that these grammars or schemata provide the hidden intervening variables. It may be said, however, that those of us who are inclined to think so cannot point to any massive, carefully coordinated set of experiments such as those reported by Underwood and Schulz. In this sense, the book reviewed here provides the very model for psychological investigation, and it deserves to be read, not merely by those interested in verbal behavior, but by psychologists generally, for this reason if for no other.

The Johns Hopkins University

JAMES DEESE

The Psychological Record, 11, 1961, 99-102.

KANTOR, J. R. Interbehavioral Psychology: A sample of scientific system construction. (2nd ed.) Bloomington: Principia Press, 1959, xvi & 276 pp.

Although the present book is an attempt to hasten psychology to its goal of a natural science, it must be admitted that the reader will find here neither the records of one quarter of a billion responses made by a biped (feathered or featherless), not a hopeful account of the workings of a wished-for nervous system, nor even a mathematical description of how college freshmen confound game-theorists by failing to maximize their chances of success in a guessing situation. While Professor Kantor recognizes the importance of all these things, he demonstrates that neither experimentation, quantification, measurement, nor emphasis on animal research will in themselves guarantee objectivity in a science.

The present slim volume is a distillation of approximately 40 years of working and writing to the end of helping psychology take its rightful place among the sciences. While some of *Interbehavorial Psychology*

will not be entirely new to students of Kantor's work, the book is a welcome formal and refined statement and clarification of his position, first fully stated in his *Principles of Psychology* (1924).

The book is divided into five parts. Part I deals with the background and development of interbehavioral psychology. In this section, Kantor discusses such topics as the evolution of transcendental institutions, logic (system making), the interbehavioral continuum, the evolution of psychological events, and system types in psychology. Parts II. III. and IV are the heart of the book and justify its subtitle—A sample of scientific system construction. Here Kantor presents his metasystem (metadefinitions, metapostulates) and his system proper (definitions, postulates, event constructs, methodological constructs, theory and law construction). In addition Kantor offers illustrative and formal examples of psychological subsystems such as physiological, psycholinguistic, learning, psychophysical, comparative, developmental, psychotechnological, educational, and clinical. In Part V Kantor discusses the homogeneity of the sciences and thus the interrelationships that exist between objective psychology and mathematics, physics, chemistry, biology and anthropology and the advantages to be gained by reciprocal cooperation. The appendix contains two articles which have previously appeared in this journal-"Interbehavioral psychology and scientific analysis of data and operations" (1956) and "Events and constructs in the science of psychology" (1957).

In this book, as in all his others, Kantor exhibits his well-known bias toward natural events. He insists that the advancement of phychology as well as any other science requires both freedom from established cultural institutions and an attachment of interest to original events so that scientific constructs can represent this immediate contact with events. He is admittedly not satisfied with the current development of psychology; the book opens with the melancholy assertion that it still seems necessary to emphasize "the fact that psychological events are in all respects as natural as chemical reactions, electromagnetic radiation, or gravitational attraction" (p. vii). Thus in the present volume, Kantor continues to exercise, in the name of Objectivity, the shoal of spooks that still embarrass psychology. He is highly critical of some of the turns psychology has taken in its quest for the goal of objective science, and he is not inhibited in expressing his objections. He, in fact, leaves few turns unstoned. For example:

On classical behaviorism:

"... the behaviorist with his roots in biological science, threw away the mental half of the constructs of his predecessors. This we might call an *adjustment* to dualism, not a fresh start." (p. 5)

On current behaviorism:

"... the behavioralistic system imposes upon all psychological data constructs derived from animal conditioning." (p. 14)

On experimentation:

"Experimentation is not sheer manipulation." (p. 97)

"Experimentation is not arbitrary procedure." (p. 97)

On the internal-external world dichotomy:

"The ideas or reactions, presumed to be projected from a mind, are organized into stimulus objects and a complex world structure. In short, the idea of a rose becomes *the rose*. Both science and the rose are the losers thereby." (p. 9)

On psychophysics:

"Those who describe the psychophysical situation as a procedure for determining the nature of stimulus objects display a traditional mentalistic bias." (p. 124)

On the sometime misapplication of the interpretive principle which leads to the quest for the unscientific goals of certainty, fixity, and ultimacy:

"This quest, for instance, has led to the search for deductive principles and the arbitrary preoccupation with symbolical and mathematical elements and systems.

"As we know, the results of this kind of thinking are the narrowing of the domain . . . Eventually the interpretive principle dominates events and becomes a Procrustean instrument to make them conform to the system." (p. 140)

On animal psychology:

"The investigation of animal-behavior yields only data and laws concerning the particular organism studied." (p. 119)

On brain models:

"Brain models are analogical and misleading.

"Brain theorists have recently used analogies borrowed from servomechanisms (positive and negative feedback loops), computing machines (coding and storing messages). and complex automatic telephone systems. No apparatus or instrument analogy can do more than satisfy the whim of the originator." (p. 115)

On mathematical models:

". . . statistical or mathematical models tend to restrict and reduce events to simple processes which may be neither typical nor important. Restricting and reducing models incline their constructors, for instance, to deal only with such events as conditioning, verbal responses, maze running, and elementary discrimination, while learning is reduced to latency or readiness to act, rate of performance or the like." (p. 138)

This small sample of statements, although quoted out of context, testifies to the severity of Kantor's strictures concerning current psy-

chology, and suggests the fact that few readers will be neutral with respect to this book. Since Kantor's arguments are explicit and well-buttressed, the reader may be drawn into true linguistic interbehavior with Kantor.

While Interbehavioral Psychology is highly critical of much of current psychological work, it does offer constructive suggestions that might well be adopted by all psychologists. Nearly a century ago, Matthew Arnold suggested using poetry of the great classics, Homer, Dante, and Shakespeare, as a sort of "touchstone" to gauge the worth, the "truth and seriousness" of other poetry. Contemporary psychologists, regardless of their specialties, might do worse than to adapt this method to their own use. Since freedom from cultural impositions and an adherence to natural events have in the past been difficult for us to obtain, it would be appropriate to compare our current job-whatever it is—with the interbehavioral system to determine whether we are being as naturalistic as we might be. Since Kantor has spelled out definitions, postulates and theorems for most of the traditional areas of psychology, e.g., psychophysics, learning, clinical, etc., the possibility of selfcorrection toward the goal of completely objective treatment is much advanced.

One word of caution: For those who are swine, it must be admitted that Kantor remains a forbidding retainer of pearls; much of the book will be tough going for the uninitiated, e.g., "Contrivance in learning is best illustrated by autogenous and hetergenous tutelage" (p. 129). However, the total absence of the fatuous and the brummagem should alone be enough reinforcement for the careful perusal of the book.

In an earlier book, Kantor asked, in commenting on the lapse into solipsism of a highly respected logician of science: "Quis custodiet ipsos custodes?" If nominations are in order, I will offer the name of Professor Kantor.

Ohio University

GILBERT R. JOHNS

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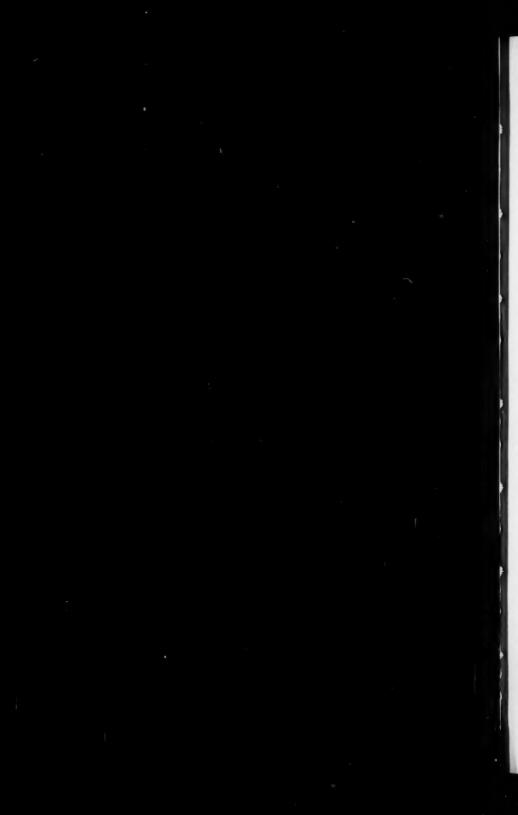
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